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VOLUME VI.

THE HISTORY OF PSYCHOLOGY

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The science of psychology essentially reflects in its development the way the human mind has been able at various epochs to apprehend itself. The thought of any object is simply the conscious construction of that object; and this is as true of the sort of object—the mind—with which the science of psychology deals as of the object of any other science. As long, for example, as animistic views prevailed, a thorough-going positivistic treatment of the objective world was impossible; for the object constructed was not subject to regular law nor continuity of transformation and change. So, also, as long as the animal body was considered an exception to the positivistic process, biology could not be a thoroughly developed natural science; for its object was a centre of capricious and mystically motived

changes. This is true of psychology, and more emphatically. For the object of the science of psychology is the mind, the object which it constructs from its own experience; that is, its object is just its own positive view of itself. We are accordingly led to see that the history of psychology is the history of the stages or modes of the evolution of reflective consciousness of self.

I. *Greek Psychology*

The evolution of psychological views among the Greeks is capable of fruitful interpretation from this point of view. The earliest views were necessarily those possible at a period of which the dualism of mind and body—self and external world—had not been achieved. The so-called "materialists" of Greece—who, just for the reason now given, would better be called "protists," "pro-noists," "projectivists" (I shall use this last term), or something of like import,—looked upon nature as the "one," "the undefined," a moving labile object (water, air, etc.). And it is characteristic of their views that they did not—because they could not—go on to make distinctions and differentiations in the lines of later more mature reflection. The period of their thinking in the history of opinion corresponds to the early adualistic or "projective" period in the individual's personal development. The individual has a certain objective mass of material, "protoplasmic" in a figure, in which the dual reference to subject and object is not yet attained. The world, to such an individual, is one of "first-appearance"—not of matter and mind, nor of anything else which gives an antithesis of poles of reference. So the early thinking of the race was in this sense unreflective. The process of its theoretical interest did not lay apart its material in substantial categories; but it answered the question "what?" by the assertion of the sort of predi-

cates which were its possible objective constructions at that stage.[1]

The positive character of this first period, however, shows the transition motive to certain later dualisms: the character of animation, movement, change. In this respect, the Ionics suggest a further movement in the child's development. The immature reflection of the individual finds, in the perception of animation and capricious movement, the road toward a solidified and concreted dualism. Through this type of reflection the world-circle closes in somewhat upon the personal centre. It neglects the fixed, changeless, inanimate things of the world, as in so far unexistent or hypothetical. In respect to them, the senses deceive. So in the thought of Heraclitus and Parmenides the becoming or change principle played its rôle, and the Greek mind began its career toward a form of dualism in which the "fixed" was of logical or contrast value, mainly, not an objective category.

In this general epoch, the "projective," in the development of Greek thinking, we must place also the νοῦς principle of Anaxagoras. It was a principle in the line of the vitalistic or change hypothesis; and it remained, indeed, only a postulate of order, movement, immanence in the world. It was not a subjective, nor yet an objective (a subjective) principle. So far as it implied a dualism, it was one—that predominant one in Greek thought—of matter and form, not of subjective and objective. It may possibly be considered as in so far an unreflective anticipation of Aristotle's biological point of view,—so much, in-

[1] This is not to say that the adult person himself—for example, such a thinker as Thales—was not self-conscious, and did not deal practically with the problem of self *vs.* things; but only that, in his reflection, he did not segregate the elements of his one general experience in explicit dualisms, nor consider the objects in the two spheres of practical experience as separate and distinct.

It may be explained here that I use the term "object" (and its adjective form "objective") of *any cognitive construction whatever*—anything that may be known or thought about.

deed, is possible,—but it was not in any sense an anticipation of the subjective point of view from which a science of psychology could isolate its peculiar matter. This accounts, no doubt, for its unfruitfulness in later thought.

The real isolation of the subjective or "inner" seems to have begun with the Atomists, Leucippus and Democritus, in their famous doctrine of the relativity of the sense qualities.[1] This intuition led perforce, just as the same type of phenomena—the relativeness and deceptiveness of qualities, colors, odors, etc., in things—leads the child, to the wider question whether the "inner" is not a sphere to be distinguished from the "outer". Indeed, in Democritus this antithesis is actually and fruitfully made. His other great doctrine, that of the "atoms," was thus made possible, and has remained possible for all time; for by definition the "outer" had to be stripped of those relative and ambiguous predicates which had embarrassed earlier speculation. The atoms could do their work in the body of external reality; and the mind could do its separate work of knowing that reality. This was a real advance upon the doctrine of "elements" as held by Parmenides and Empedocles.

The subjective postulate, thus once arrived at in the individualistic sphere of sensation, was to be carried out in the general sphere of truth by the Sophists; indeed, it was forced upon them by the social and intellectual conditions which made men Sophists in their generation. In the Sophists began the play of certain forces akin to those which we find enormously germinal in the narrower sphere of the individual's personal growth. And in this our present method has further justification.

The growing consciousness of personal quasi-subjective detachment from the world of impersonal things comes to the child through processes analyzed variously into motives

[1] *Cf.* Gomperz, *Greek Thinkers*, vol. I, pp. 320 ff.

of conflict, imitation, invention, discussion (and from the psychic point of view, introjection, absorption, realization) —a give-and-take or dialectical process between the individual and his fellows. In it all the essential fact of subjectivity in the actor's thought of himself and others comes to birth. The actor becomes an agent; the observer, a creature of reflection; the spontaneous thinker, a possible amateur psychologist.

All this appears, there can be no doubt, in the Sophistic movement; and out of it, indeed, the first race-psychologist was born,—Socrates. In the views and methods of Socrates are focused the rays which are to burn inward to the core of the human self. This appears true of Socrates in the following precise points.

(1) The Sophistic principle, *homo mensura omnium* (Protagoras), formulates the thought of an active and constructive centre in the individual. The individual's or human nature's reaction to the world gives all the measure there is for things. In Socrates this principle was developed in an anti-individualistic or social sense.[1]

(2) The contrast between "opinion" (δόξα) and "reason" (νοῦς) sharply brought out by the dialogue method in the hands of the master, Socrates, and developed by his disciples, now becomes more positive.

(3) The view that truth is in general a thing of thought in so far eternal and immutable,—not, as in the earlier transition stage, a function of a principle of change essentially indeterminate in character. This is the germ

[1] Against the individualistic interpretation for the Sophists generally, especially Protagoras, see Gomperz, *loc. cit.* I, 451 ff.

It is confirmatory of the parallel made in the text between the Sophist's and a stage in the individual's thought to note that Socrates' position was not in its nature individualistic, but was reached and maintained in the midst of social opposition and discussion. The Socratic method was a social dialectic, or give and take. I do not know of any adequate exposition of the social—political, religious, etc.—factors which produced the Sophistic movement; but an account of a later analogous period—the rise of the Post-Aristotelian schools—is given in admirable terms by Caird in his *Development of Theology in the Great Philosophers*, II, lect. XV.

of Plato's "idea" in which reality becomes explicitly an ideal postulate.

(4) In Socrates the way is opened to the form of dualism of mind and matter found in Plato's doctrine of matter (ὑλή).

Not stopping to develop these points,—time does not allow,—we may still say that Socrates was mainly a Sophist, not a clear subjectivist. He reached subjectivism only so far as it was involved in a dualism of the general (truth) and the particular (appearance), and that in an experimental and controversial way.[1] He did not realize the thought of mind as a psychic content in distinction from body.

Had Plato been possessed of the scientific interest, this distinction might have been made then and there; for Plato deduced a principle of matter. But, like Anaxagoras, with his postulate of mind, Plato's "matter" remained a logical contrast principle, over against "form,"—a particular over against the general,—not a concrete reality; and the philosophy of reality was to remain a rule of vibration between logical poles, rather than a synthesis of reflection.

So far as a science of psychology goes, Plato must be classed with Socrates in what we may call the period of "experimental subjectivity."

In Aristotle, no less than in Plato, it is the outward movement of thought into reality that has the emphasis, not the development of the subjective as psychic. This movement is that described in modern genetic psychology as "ejection": the reading of the subjective into the external and the interpretation of the latter in terms of some aspect of the world of thought. This reached its clear statement in

[1] The way which, when illustrated in the individual's development, I have called the construction of a "semblant" object—a matter of psychic experimentation with materials, akin to the child's playful and esthetic imaginative constructions.

Plato's doctrine of "ideas," that is, so far as the "idea" itself was defined. It required a theory of the idea, however, only so far as that conception was to serve the metaphysical purpose. It did not require, nor did it receive, independent treatment, as the object of scientific research or even as content of consciousness. The dualism, however, was only a meditating phase of the return to a deeper monism or idealism: that of the unity of the particular and the universal. And in Aristotle, whose scientific impulse was strong, this reading of the subjective into the objective remained—in the doctrine of matter and form—a way of accounting for the organic character of the presented and objective world. It did not become a way of detaching the subjective. This is to say that Aristotle's point of view, in discussing the facts of mind, is more biological than psychic or psychological. Mind has definition as the form of the animal body; and while this implies a reciprocal definition of body, —as material for the realization of form,—nevertheless the emphasis is not on mind as such.[1]

Aristotle illustrates, indeed, an important fact in the history of science in general: the fact that positivism may be embodied in a scientific method before the criticism of the material is well advanced, and that the sciences of the objective order are usually well along before the corresponding sciences of the subjective order attain their emancipation. The reason of this limitation in the case of Aristotle appears when we turn again to the parallelism between the individual's and the race's growth in self-consciousness. The embodiment of the thought-content in things, by "ejection," or, as the anthropologists say, by "personification," suffices for a theory of the world which is animistic and vitalized,—for hylozoism, that is. But this

[1] This is not to say, of course, that Aristotle did not make many valuable contributions to empirical psychology; he did. But still it is true that he did not develop a distinctly psychic method of treating consciousness.

does not go beyond Plato. The next step is to reach, with Aristotle, a naturalism of the objective order, by the correction and limitation of the animistic concept. This the individual does on his part by the return movement of his thought, whereby he reabsorbs a body of predicates into the "inner" sphere. The psychic becomes, by this movement, the theatre of the more lawless, capricious, and unmanageable phases of appearance, and the world order remains what is left,—the regular, the manageable, the lawful. The fixed, before neglected, now becomes the essence of things. It is, no doubt, a practical distinction at first, and only afterwards becomes the subject of that theoretical interest which develops its positivism first of all in the objective realm. So the rise of science of the objective becomes possible. But not yet, evidently, can the psychic find corresponding treatment, as law-abiding and uniform in its movements; for if the inner sphere be constituted just by the segregation of materials in so far practically unmanageable, the theoretical treatment of them is thereby baffled; and a science of these contents must await the rise of a reasoned positivism of the innner life.

It is necessary to point this out, for it explains certain negative aspects of later historical movements—and why psychology as a science of content was so late a growth. In two later world-epochs, in particular, and in their respective world-thinkers, something of the same situation presents itself. I refer to the rise of modern dualistic philosophy in Descartes, and the rise of Positivism of the stricter sort in Auguste Comte.

II. *The Dualistic Transition*

The transition to Descartes was made through the Stoics and the theologians of the Christian Church. The Stoics, reacting against the practical individualism of the Cynics

and Cyrenaics, reached the concept of a sort of general selfhood which guaranteed law and order and virtue. This was a practical and eclectic rather than a reasoned attempt to overcome the dualism of their immediate predecessors.[1] The church theologians reasserted an individualism, but to them the individual became spiritual.

In these precursors of Descartes there was worked out a genetic motive which is unmistakable also in the individual's development: I mean the advance or progression from a dualism of "inner-outer" to one of "mind-body"—from what may be called a distinction of attributes to a distinction of substances. The individual proceeds, in his generalization, to carry over the physical part of his own person—separating it substantially from the psychic part—to the side of the "outer" as such. It is only when he is able to do this, *and does it,* that the dualism of mind and body is anything like complete. The substantializing of the mental principle which has so far proceeded by certain curious stages—being variously a refined physical something, a breath, the limiting notion and form of matter—now finally becomes the hypostatized substance which bears the psychic qualities. The substance soul does finally become logically detached, but mainly for theoretical and doctrinal purposes;[2] for even then soul and body remain in so far attributal to each other, that either can be predicted on occasion as either cause or effect with reference to the other. This was notably true in the entire church development; and the view is still dominant in theology.

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[1] *Cf.* Caird, *loc. cit.,* lect. xvii.

[2] The earlier crass doctrine of transmigration, as in the Orphics and in Empedocles, did not involve a reflective dualism; for the soul was not defined as a principle. When the dualism arose, however, such views availed themselves of so much support, just as modern theology supplies a doctrine of immortality in support of the early anthropological belief in a world beyond. Put in psychological terms, we may say that such early religious and anthropological views were object of practical and, in some cases, esthetic interest, but not of the sort of theoretical interest which leads to philosophical inquiry.

This cause and effect bond is the last one that remained to be loosed.[1]

In Descartes, for the first time in the history of thought, certainly of Occidental thought, is the psycho-physical problem specifically set in the form of the conception of a natural relation between mind and body, considered as two separate substantial principles. The problem becomes: what is the relation? It assumes not only the dualism of the two terms, but their actual separation. Descartes not only reaches such a dualism, but he sets up the full relational problem of mind and body. And further, he identifies the spiritual principle with "inner experience" or "thought." He is in advance of the church philosophy in this important respect, that while, to the latter, it was a problem of *separating mind and body,* to Descartes it was a question of *bringing them together again.* Descartes said that interaction was impossible; and the theory of preëstablished harmony was the alternative.

Why, then, it may be asked, did not a purely naturalistic psychology begin with Descartes? For much the same reason, I surmise, that it did not begin with Aristotle: because Descartes did not conceive the inner principle, the soul or thought, in terms of continuous and lawful change. Just in this was it contrasted with body. Extension is the sphere of geometry and physics; thought is the source of spiritual manifestations; and these two domains of fact, though parallel, are essentially heterogeneous. That this is true of Descartes is proved historically; just as the corresponding fact comes out in the comparison of Aristotle with Socrates. In each case a monistic idealism followed, not a scientific naturalism. Socrates was followed by Plato, Aristotle by a new mysticism, while Descartes led right

[1] I have pointed out elsewhere (*Psychological Review,* May, 1903) that the case of mind and body is the last instance of that sort of commingling of substances and forces. It is present in all the forces involved in "interaction" theories.

on to Spinoza. In each case, we find an attempt to transcend the specific form of dualism of its own period.[1]

III. *The Postulates of Modern Scientific Psychology*

From the preceding exposition, I may venture to draw certain inferences of a negative sort: statements of what the thought of the earlier centuries lacked; and follow that with the positive characters belonging to the nineteenth-century science.

What the earlier thinkers lacked, then, was (1) a full naturalism in their point of view: a naturalism which could follow only upon a critical dualism of mind and body. Grant the dualism of inner and outer, take the further step to that of mind and body, then—and this is the needful thing for naturalism—admit the oneness of the knowledge of nature as a whole in the face of the cleft in nature which the dualism postulates. The thinkers we have been considering did not achieve this last step. They worked out their theoretical interest by establishing a philosophical solution of the dualism, or, on the other hand, resorted to an esthetic handling of it.

(2) They did not achieve a positive way of treating all data as material of knowledge as such, material to be progressively systematized and enlarged by research. The former is the full scientific point of view; the latter is its method and instrument.

What modern psychology has in addition is just the something that these early thinkers lack:

(1) *Naturalism,* or the view that all events or phe-

[1] It is an interesting point that in each such case, the supposed reconciliation is not logical, but, in a broad sense, esthetic: the motive in Plato is poetic, in the Post-Aristotelians it is mystic, in Spinoza, it is religious,—a matter it would be well to expound in its own place. It has its parallel, moreover, in the individual's mode of treating his dualisms, *i. e.*, by the construction of objects which are valid from esthetic points of view. This is, I think, the normal genetic outcome.

[2] It should be noted that I speak of *scientific*, not of *philosophical* naturalism and positivism.

nomena whatever are part of a natural order, and are subject to general and ascertainable rules of sequence.

(2) *Positivism*,[1] or the view that a methodology—a theory and practice of method—of research is possible, for the discovery of the rules or laws which govern the sequences of the natural world.

Both of these scientific postulates hold for psychology. They have long been established in the physical or exact-quantitative sciences; they have been slow of formulation in the biological sciences; they are only beginning to have adequate recognition—especially, the second of them—in the mental and moral sciences. It is the characteristic feature of nineteenth-century psychology, that it has developed the first of these postulates fully and the second partially.

IV. *History of Nineteenth-Century Psychology*

The nineteenth century opened at a natural pause in the evolution of theories about the mind. In the flow of the great currents, certain eddies had formed late in the eighteenth century. The dogmatic movement in Germany had passed over into the critical; and Kant had attempted a new esthetic reconciliation of the dualism of inner and outer. The Kantian psychology or anthropology is essentially a renewed subjectivism—that is, so far as it is "critical." Neither scientific naturalism, nor positivism in the sense defined above, profited greatly from the work of Kant. Indeed the explicit attempt to refute Hume—to go no deeper—throws the weight of Kant as authority on the side of an essentially obscurantist attitude toward facts. Note the arguments in favor of *a priori* space and time, which very little careful observation would have materially modi-

[1] It should be noted that I speak of *scientific*, not of *philosophical* naturalism and positivism.

fied. And historically Kant led the way to what Höffding calls the "romantic movement," from Fichte to Hegel.

Again in France an impulse was asserting itself away from the materialism of the sensationalists toward the naturalism of Rousseau. Rousseau's recognition of the psychic involved a truer naturalism than the view which denied the life of ideas and of all higher functions in favor of a sense-process materialistically interpreted. Neither Rousseau nor Condillac, however, combined both the two postulates.

In England a science of psychology was emerging at the opening of the nineteenth century. Lock had broached a subjective naturalism, which the French sensationalists, as I have just intimated, developed on one side only. Hobbes was a positivist in much the same sense for our purposes as Comte. But in David Hume the two requirements of a true science of psychology were consciously present. Hume treats mind as a part of nature,—this is naturalism,—and he also works at the problem of discovering the laws of mental change by actual observation,—this is positivism. He is justified in both by his results; he is further justified by his extraordinary historical influence.

If then we are justified in saying that David Hume is one parent of the science of psychology,—in the sense of the word that places this subject in line with the other natural sciences both as to its material and to its method, —then we have to look for the other parent, I think, to France. Dropping the figure, we may say that, in Rousseau, France contributed an essential moment to the development of the science. Possibly this contribution should be called the Rousseau-Comte factor; as possibly also the British contribution should be called the Locke-Hume factor.

The influence of the Rousseau-Comte factor, which is

to-day more undeveloped than the other, but is now becoming fertile, may be shown by an appeal again to the analogy with the individual's growth in personal self-consciousness. And as intimation of my meaning, I may refer to the Rousseau-Comte *motif* as the "social" or "collectivist," and to the Locke-Hume *motif* as the "personal" or "individualistic."

Taking up the genetic parallel, we may remark that the development of the positivistic postulate by Locke, Hume, and the Mills, *in an individualistic sense,* has proved inadequate, so far as it claims to exhaust the psychic matter. In the development of the individual the rise of the thought of a separate personal self is a late outcome of reflection. The early stages of dualistic thought are in so far social that the mind-body dualism is an abstraction in both its terms. Mind is many minds; and body is many bodies. The material of self is collective and distributive, not unitary nor individual. The child thinks self as a term in a social situation.

If this be true, the science of mind must be one in which the abstraction of an isolated individual mental life is to be used as an instrument of method rather than as a truth of analysis and explanation. And there should be a science of psychology in which the material is, so to speak, social rather than individual. This point has been worked out only in recent literature, but its advocates may find the source of this type of view in the French thinkers now under discussion.

Besides these two great movements, credited respectively to Great Britain and France, modern naturalistic psychology has had two important impulses. The first of these came about the middle of the century in the rise of the evolution theory, and from the side of biological science; the other from German beginnings, and from the side of physical

science. I shall speak of these respectively as genetic psychology, finding its pioneers, Lamarck and Darwin, in France and England, and experimental psychology, founded by the Germans, Fechner and Lotze.

The various factors now distinguished may be taken up briefly in turn for consideration. I shall treat them under the two larger headings already set forth: *Naturalism*, comprising (1) the British movement called above the Locke-Hume factor (empirical psychology), and (2) the French-British evolution movement (genetic psychology); and *Positivism*, comprising (1) the Rousseau-Comte movement (social psychology), and (2) the German experimental movement (experimental psychology).[1]

Before proceeding, however, it may be well to give a résumé of principles,—the platform upon which the entire development is projected. This platform is that of cognitive and reflective self-consciousness of such a sort as that which the individual has attained when he thinks of his inner life as a more or less consistent unity, passing through a continuous and developing experience: a self different from things, and also different from other selves; yet finding its experience and exercising its functions in closest touch with both. And furthermore this touch with things and persons is so close that whatever his reflection about himself may lead to, he accepts the facts, (1) that the world

[1] These two headings are indeed not exhaustive nor mutually exclusive. The viewpoint respecting the material cannot fail to influence the method; nor the method the selection of material. For example, the Rousseau-Comte current is a direct gain to naturalism no less than to positivism; and the opposite is true of the Locke-Hume movement.

The scientific treatment of mental diseases is also a most important matter, which should be classed under positivism or positive method. It is not within my province—nor is the time ripe, I think—to estimate it. Its development is one of the great tasks of the twentieth century (*cf.* Meyer, *Psychological Bulletin*, May-June, 1904, for an exposition of present-day tendencies and theories).

As it happens, it fell to the present writer to draw up a report on psychology for the other great American exposition, that at Chicago in 1893. That report, entitled *Psychology, Past and Present* (published in the *Psychological Review*, and now incorporated in the volume *Fragments in Philosophy and Science*), goes into greater detail respecting recent movements and literature, with special reference to conditions in the United States.

as a whole *includes himself and others in its larger uniform processes,* and (2) that the methods of its treatment of him *through his body,* are also his methods of handling it. The individual must be, that is, *first,* a somewhat careful naturalist, and also *second,* a somewhat skillful positivist; and it is only when there is the reflection of *this sort of self-consciousness into the scientific endeavor of the race* that there comes a time ripe for a truly scientific psychology.

IV. *Nineteenth Century Naturalism*

British Empirical Psychology. The empirical movement reasserted in John Locke the subjective point of view reached in the dualism of Descartes. Furthermore, it attained in David Hume the return movement from a pure naturalism of the objective only to a corresponding naturalism of the subjective. Locke's subjectivism is seen in his doctrine of primary and secondary qualities, in which he renewed the relativity of Democritus and the Cynics, and in his polemic against innate ideas. Hume's subjective naturalism appears in his entire work. Hume's theories of ideas, belief, substance, cause, all testify to his complete absorption in the thought of the psychic as a law-abiding and continuous flow of events.

The most explicit result of this point of view appeared, however, in the theory of Association of Ideas, upon which the school of British empiricists founded their psychology. James Mill, J. S. Mill, Thomas Brown, and Alexander Bain are the figures which are drawn large upon the canvas of associationism in the nineteenth century. The theory of association, considered as a formula of general explaining value, was epoch-making historically, inasmuch as it was the first general formulation made from the new point of view.

In France, something in some degree analogous appears

in the writings of Condillac and his associates before the voluntaristic reaction of Maine de Biran and Jouffroy. The postulate of sensation was indeed a naturalism, as has been said above; but it was not motived in strict philosophical neutrality, nor did it issue in a general formula. At the same time it served to establish the Lockian tradition on the Continent, and to furnish a shibboleth which, though destructive enough from other points of view, nevertheless helped to clear the way to a saner empiricism. It should be noted, too, that there were in Germany sporadic intimations, and more, toward a fruitful naturalism; but that these remained without great influence—notably the remarkable work of Beneke—and had to be reformulated in later times, shows that, as matter of fact, the naturalistic movement did not receive any indispensable support from Germany.[1] Beneke's advanced positions, it is fair to add, are only now becoming generally known as anticipations of certain important genetic principles.

The outcome of this great British movement is an established empirical tradition. The gain is seen, on one side, in the soil tilled for the sowing of evolution seed; it appears again in the established spirit of patient research which is the life-blood of science. In Alexander Bain we have the summing-up of the results for the whole mental life; as in Herbart, in Germany, we find them illustrated in a new Intellectualism; and in Herbert Spencer, their further development on a Lamarckian platform. In Spencer, it is true, the psychological point of view served the need of a larger philosophical purpose; but he shows that the naturalistic habit of mind had become so fixed that the association psychology could be recast on evolution lines, while claiming still that violence had not been done to its

[1] Indeed, this might be put more strongly: for the era of the Enlightenment in Germany brought a reaction toward the more mystical evaluations of experience based on feeling—*e. g.*, in Tetens and Schleiermacher.

essentially empirical spirit. A later author, in whom the positivistic method is well realized, but in whom the genetic spirit is not fully developed, is William James; and still another, who will be named below as one of the pioneers of the experimental psychology, Wilhelm Wundt, is not only not genetic in his naturalism (being neo-vitalistic), but has also a corresponding limitation upon his method, in spite of its positivistic claim (being somewhat obscurantist in his demand that psychology shall yield support to a philosophical voluntarism).

French-British Evolutionism; Genetic Psychology. The rise of the genetic evolution theory in biology supplied the direct motive to a psychology. Lamarck himself recognized the psychological factor in one of his general principles—that in which he formulated the function of mind as effort, struggle, etc., in modifying the organism to accommodate it to the environment. The explicit application, however, of the Lamarckian theory was due to Herbert Spencer, in whose work we recognize a conscious attempt to work out an evolution theory of mind, as a branch of general cosmology. It is interesting that it was in the same generation, indeed in the same decade, that those other Englishmen, Darwin and Wallace, gave both biology and psychology alike an impulse which has established a genetic science. For Lamarckism is not positivism; only in Darwinism did a thorough-going positivism of method supplement and correct the naturalism of Spencer and Lamarck. The contribution consisted in the extending to mind of the methods of positive and comparative research, and the formulation of a principle, that of natural selection, which established genetic continuity and by which research has since been directed and controlled. It is somewhat remarkable that Lamarckism never secured the hold upon the minds of psychologists that it did upon those of biologists;

and the progress to Darwinian positivism has had real reinforcement from workers in our science.

Now—at the end of the nineteenth century—the genetic principle is coming into its rights. It has done most service hitherto negatively, in its antagonism to a psychology exclusively associational, on the one hand, and to one exclusively structural, on the other hand. The earlier science was debtor, in its structural concept, to physics; it was a positivism of the atomistic or agenetic type. The latter is debtor, in its functional concept, to biological science; it is a positivism of the developmental or genetic type. However fruitful the atomistic, structural psychology has been, it has had its word, and it is not the final word. A great era of research is upon us in the treatment of consciousness as a thing of functional evolution in the race, and of personal development in the individual. This general psychology of the future has been prepared for in the physical mode of psychologizing, just as the general biology of the present was prepared for by the anatomical science of life which preceded it.

Among those whose names should be mentioned as contributing either to the Lamarckian or to the Darwinian form of the genetic principle are Haeckel and Weismann in Germany; and among those powerfully aiding its acceptance in their respective countries are Ribot in France, Morselli in Italy, Romanes and Huxley in England, and John Fiske in America.

V. *Nineteenth Century Positivism.*

French Positivism; Social Psychology. In France the progress of naturalism, in matters psychological, was much more rapid, and its victory more complete, than in England and Germany. This difference is due, I think, to the different attitudes taken in these countries respectively

toward the theory and practice of religion. In France, the theological bias and restraint, in which a certain conception of the mental principle was involved, were done away with before and during the revolution; and a positive scientific method was resorted to, to replace the theological—as witness Comte's actual attempt at a religion of humanity. In England, Germany, and America, on the contrary, while the growth of naturalism has gone on apace, the actual realization of scientific method has been slow and difficult. Such a step involves the giving-up of vitalism and the theory of interaction of mind and body, together with other formulations in which the theological spirit has lately taken its stand.

In Auguste Comte we have a thinker whose dualism was ripe for a scientific psychology, but who nevertheless failed to achieve the point of view of law-abiding or subjective change. Comte was *assez positif* in his claim. He took up the problem of an independent science of psychic processes; but from failure to recognize the subjective as such, denied its possibility. His objective monism is seen in his view that it is through the objective or positive series of facts, biological and social, that the psychic series is to be done justice to,—classified, arranged, and explained.[1] It is the reverse swing of the pendulum to that of subjectivism, though from a different theoretical support. It does not solve the dualism; as the idealistic monisms of Plato and Spinoza did not. And it parallels practically the same stage of individual reflection as these systems: that which recognizes the futility of the half-mature dualisms of practice and common sense. But in Comte the practical and the methodological were prominent, and he was urged on to justify the sort of naturalism in which

[1] His inconsistency is seen in his appeal to the subjectivism of Kant's relativism of knowledge to refute metaphysics, while using the objective order to refute the subjective point of view of Condillac and the spiritualistic school.

these two motives issued. This he did by asserting the essential fragmentariness and capriciousness of the psychic as such; while he should have held to a larger naturalism, in which the external and the psychic each develops its own positive method.[1] Of course it is no reconciliation of two terms to deny one of them; and such a procedure has not the merit of esthetic synthesis which we find in the great monisms. But nevertheless, the assertion of the universal claim of positive method was of the first importance: it carried forward one of the great naturalistic movements of history.

While the fruitfulness of the positivism of Comte was thus in science in general, not directly in psychology, yet it was only his personal convictions that hindered his coming into the psychological heritage as well. As it was, the spirit of his teaching awaited its working-out in a later generation. It was to the profit of sociology; for his negative answer to the question of positive psychology was possible only because of his affirmative answer to that of social science. The positive bearing of Comtean positivism comes out, therefore, in two ways: first, as announcing a general method, and second, as preparing the way for a social psychology which should reconstitute part of the domain assigned to sociology—that of psychic and social experience—in a separate science.

As to this latter undertaking,—the isolation of the content of social psychology,—the requirement had already been met, in spirit at least, by Jean Jacques Rousseau. In Rousseau, to whom French naturalism owes its main impulse, we find two contrasted and in a sense opposing points of view, one positive and the other negative. These together tended to the segregation of a certain sort of ma-

[1] This was done by the school of English positivists who followed Comte in his attitude toward metaphysics.

terial. These positions were, first, the positive "return to nature," which took the form of individualism in politics and education (in *The Social Contract,* and *Emile*), and, second, the theory of the "general will," which opened the way for a new collectivism, whenever its implications for social psychology should be brought out.

These positions of his predecessor might have led Comte into a truer view, and have brought about the establishing of a social psychology,—a science of the "general will"—in the spirit of the motto "back to nature." But this, as we have seen, Comte did not realize.

Undoubtedly, however, there is a profounder reason for the immediate unfruitfulness of the work of Comte—and this is my justification for dwelling so long upon it. Pursuing the method employed above, we may still recognize the requirement that the science of mind follow the genetic stages of the individual's growth in self-consciousness. With this cue, we may say that it was impossible that a psychology of social collectivism could be established before a theory of psychic individualism had been fully worked out. The individual is, indeed, truly a social person from the start; but this he himself does not recognize until he has lived through a period of strenuous unreflective self-assertion. Moreover, even then this consciousness of his social place is not in itself the adequate impulse to the theoretical interest to explain it. So social psychology, which embodies just such an interest, must perforce await the development of individual psychology and then serve to supplement it. We are able to see this now, inasmuch as we are only now realizing the transition from the latter to the former; and it is for this reason, also, that we are able to see why it was that both in France and in England the repeated claim of collectivism, both social and political, was negatived and outlawed. Hobbes must yield to Locke,

Comte to Mill and Spencer; and only after these latter could Bagehot, and Stephen, and Tarde arise, if, indeed, the renewed collectivism was to have a psychological foundation worthy of the name. And it is equally true that it is only as we work out in the genetic processes whereby the reflective social self of the individual justifies its right to succeed the individualistic, that we can expect to see how society can rationally hope to reconstitute itself as more than a group of competing individuals. For having begun this work later, psychology, notably in France and America, deserves praise. But it can succeed only as it maintains both the naturalistic spirit and the positivistic method of Comte.

German Positivism; Experimental Psychology. The establishing of laboratory psychology is usually and rightly accredited to the Germans; but it is not so usually seen that this work does not involve a new point of view. On the contrary, it is the culmination of the positivistic movement sketched above. It not only admits the place of mind as a part of nature, but it suggests the employment of the methods of physical and physiological science. It arose in Fechner's attempt to discover the law of connection between psychic and bodily events. Such a law once made out, research would be guided and also controlled by its recognition.

Apart from the fact that the attempt failed, so far as Fechner's investigation was concerned, the importance of the conception cannot be questioned. A later formula—that of psycho-physical parallelism—is indeed truer to the ideal of a working positivism, just from its negative and colorless character. But ignoring points of controversy, we may still say that many fruitful researches have been carried out in this field; and disabusing ourselves of too great optimism, we may still count laboratory work as a part of

the heritage bequeathed to the twentieth century. No doubt we are to see fruitful formulations under the rule of which great discoveries are yet to be made. Together with the actual founder, Fechner, we should name Lotze as also a pioneer in experimental psychology, and Wundt as an effective builder upon their foundations. Other great names in this connection are those of Weber and Helmholtz.

VI. *Prospects*

In conclusion, it may be deemed proper to set forth the probable lines of development of psychological inquiry in the opening century.

In the first place, it is clear that both naturalism and positivism—spirit and method—are to survive in psychology, as in science generally. And for the reading of their future development we may again appeal to the rule of individual development. Certain lines of probable advance may thus be discerned.

(1) The thought of the unity of social content is a great step toward the breaking-down of any associational or other "privately conducted" science. The psychology of the future will be social to the core; and its results, we surmise, will be revolutionary in logic, sociology, ethics, esthetics, and religion,—the disciplines which are built upon psychology.

(2) It follows that the position that the private psychic point of view is the only valid one is to grow more and more obsolete, among workers in this field. It will no longer be possible to claim that all truth about mind must be traced in some individual's consciousness, and that the laws of the science are to be those of observable psychic continuity alone. Psychic events are intertwined with physical and biological events, and their sequences involve objective as well as subjective terms. The two sciences

which will for this reason be brought into vital relation with psychology are physiology and sociology.

The two lines of development just mentioned are guaranteed by the essentially social—and by corollary, unprivate—character of our higher reflective processes.

(3) The genetic point of view will be worked out in a method of research by which genetic science will take its place beside quantitative science: psychology will become largely genetic or functional. The method in the biological sciences brought in by the theory of evolution consists essentially in the tracing-out of genetic sequences; a thing is defined in terms of what it does and becomes and of what it arose from. The anatomy of structure is only a restricted and largely descriptive branch of general biology. So psychic processes are to be understood as phases of a continuous function; their meaning is in what they do or become and in what they arise from. The analysis of a cross-section of consciousness is either descriptive, and thus barren of further results, or it is hypothetical, and in so far possibly mythological. This is the essential defect, and the dilemma of a "structural" psychology.[1]

The genetic movement is guaranteed by the current demand and need that the dualisms of partial reflection embodied in the older science be overcome. Only as a law of genetic development is realized can the postulates of self-consciousness at this period or that be justified. But the justification of one such set of postulates is, in each case, the abrogation of a former set, and the prophecy of a later set. The law of the whole series as such it is the task of genetic science to establish. It is no longer possible to rest content with a science of body in one text-book and a science of mind in another text-book, each of which claims

[1] It may be observed that even the association psychology was preferable to the modern attempts to reach a psychic atomism, and from these to construct the mental life; for the law of association deals with concrete actual units, and formulates real psychic happenings.

that no single text-book can be written from a point of view which explains the origin of the dualism of the two, and sets forth the goal at which the dualism is finally explained. Apart from private speculation, it is psychology alone which can solve this problem; since it is psychology alone in which the very movement itself by which the sciences are differentiated writes itself down as a form of reflection. The origin, the motives, the object, the goal of thought itself are just the content of psychology; psychology must become, therefore, more and more the interpretation and reinterpretation of the genetic movement of the entire thought-content.

(4) Involved in the two lines of progress just indicated, —the social and the genetic,—and also confirming our expectation regarding them, there will be a racial and comparative psychology. In racial evolution the human genetic series is objectively worked out; and in the animal world, treated by comparative psychology, the corresponding pre-human series is displayed. Here psychology will come into vital contact with ethnology, on the one hand, and with animal biology, on the other hand.

Thus described, the work of the nineteenth century in psychology has been indeed most important. It has established the science; it has set the direction of its future movement. It remains for the twentieth century to reach practical applications of its results, and to improve the methods and instruments of further discovery. The present outlook is that social psychology will be carried on in France and America, genetic psychology in England and America, experimental psychology in Germany and America.[1] And such an expression is only what may be put more explicitly in the form of the opinion that in no country is the outlook

[1] In Italy the principal currents set toward pathological and physiological psychology.

so bright for the science in all its branches as in the land of the Louisiana Purchase Exposition, of which this Congress of Arts and Science is the most interesting and perhaps the most remarkable part.

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THE PRESENT PROBLEMS OF INORGANIC CHEMISTRY

BY SIR WILLIAM RAMSAY

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To discuss the "present problems of inorganic chemistry" is by no means an easy task. The expression might be taken to mean an account of what is being actually done at present by those engaged in inorganic research; or it might be taken to relate to what needs doing—to the direction in which research is required. To summarize what is being done in an intelligible manner in the time at my disposal would be an almost impossible task; hence I will choose the latter interpretation of the title of my address. Now, a considerable experience in attempting to unveil the secrets of nature has convinced me that a deliberate effort to discover some new law or fact seldom succeeds. The investigator generally begins unmethodically, by random and chance experiments; or perhaps he is guided by some indication which has struck his attention during some previous research; and he is often the plaything of circumstances in his choice. Experience leads him to choose problems which most read-

ily admit of solution, or which appear likely to lead to the most interesting results. If I may be excused the egotism of referring to my own work, I may illustrate what I mean by relating the following curious coincidence: After Lord Rayleigh had announced his discovery that "atmospheric nitrogen" was denser than "chemical nitrogen," I referred to Cavendish's celebrated paper on the combination of the nitrogen and the oxygen of the air by means of electric sparks. Fortified by what I read, and by the knowledge gained during the performance of lecture-experiments that red-hot magnesium is a good and fairly rapid absorbent of nitrogen, it was not long before a considerable quantity of nearly pure argon had been separated from atmospheric nitrogen. Now it happens that I possess two copies of Cavendish's works; and some months afterwards I consulted the other copy and found penciled on the margin the words "look into this." I remembered the circumstance which led to the annotation. About ten years before, one of my students had investigated the direct combination of nitrogen and hydrogen, and I had read Cavendish's memoir on that occasion. I mention this fact to show that, for some reason which I forget, a line of work was not followed up, which would have been attended by most interesting results; one does not always follow the clue which yields results of the greatest interest. I regard it therefore as an impossible task to indicate the lines on which research should be carried out. All that I can do is to call attention to certain problems awaiting solution; but their relative importance must necessarily be a matter of personal bias, and others might with perhaps greater right suggest wholly different problems.

The fundamental task of inorganic chemistry is still connected with the classification of elements and compounds. The investigation of the classification of carbon compounds

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forms the field of organic chemistry; while general or physical chemistry deals with the laws of reaction, and the influence of various forms of energy in furthering or hindering chemical change. And classification centres at present in the periodic arrangement of the elements, according to the order of their atomic weights. Whatever changes in our views may be concealed in the lap of the future, this great generalization, due to Newlands, Lothar Meyer, and Mendeléeff, will always retain a place, perhaps the prominent place, in chemical science.

Now it is certain that no attempt to reduce the irregular regularity of the atomic weights to a mathematical expression has succeeded; and it is, in my opinion, very unlikely that any such expression, of not insuperable complexity, and having a basis of physical meaning, will ever be found. I have already, in an address to the German Association at Cassel, given an outline of the grand problem which awaits solution. It can be shortly stated then: While the factors of kinetic and gravitational energy, velocity, and momentum, on the one hand, and force and distance, on the other, are simply related to each other, the capacity factors of other forms of energy;—surface, in the case of surface-energy; volume, in the case of volume-energy; entropy for heat; electric capacity when electric charges are being conveyed by means of ions; atomic weight, when chemical energy is being gained or lost;—all these are simply connnected with the fundamental chemical capacity, atomic weight, or mass. The periodic arrangement is an attempt to bring the two sets of capacity factors into a simple relation to each other; and while the attempt is in so far a success, inasmuch as it is evident that some law is indicated, the divergences are such as to show that finality has not been attained. The central problem in inorganic chemistry is to answer the question, why this incomplete concordance? Having stated the gen-

eral question, it may conduce to clearness if some details are given.

(1) The variation of molecular surface-energy with temperature is such that the surface-energy, for equal numbers of moluecules distributed over a surface, is equal for equal intervals of temperature below the temperature at which surface-energy is zero—that is, the critical point. This gives a means of determining the molecular weights of liquids, and we assume that the molecular weight of a compound is accurately the sum of the atomic weights of the constituent elements.

(2) The volume-energy of gases is equal at equal temperature from that at which volume-energy is zero—*i. e.*, absolute zero. And it follows that those volumes of gases which possess equal volume-energy contain equal numbers of molecules—again, a close connection with atomic weights.

(3) The specific heats of elements are approximately inversely proportional to their atomic weights; and of compounds to the quotient of their molecular weights divided by the number of atoms in the molecule. Specific heat and entropy are closely related; hence one of the factors of thermal energy is proportional (nearly) to the reciprocal of the atomic weights.

(4) The ion carries in its migration through a solution one or more electrons. Now, the ion is an atom carrying one or more charges—one for each equivalent. Here we have the capacity for electric charge proportional to the equivalent.

(5) The factors of chemical energy are atomic weight and chemical potential; and as the former is identical numerically, or after multiplication by a simple factor with equivalent, electric potential is proportional to chemical potential.

We see, therefore, that surface, volume, thermal, electrical, and, no doubt, other forms of energy have as capacity factors magnitudes, either identical with, or closely related to, units of chemical capacity; while kinetic and linear energy are not so related, except through the periodic arrangement of the elements.

It appears, therefore, to be a fundamental problem for the chemist to ascertain, first, accurate atomic weights, and, second, to investigate some anomalies which still present difficulties. In America, you have excellent workers in the former branch. Mallet, Morley, Richards, and many others have devoted their time and skill to perhaps the best work of this kind which has been done; and F. W. Clarke has collated all results and afforded incalculable help to all who work at or are interested in the subject. Valuable criticisms, too, have been made by Hinrichs; but it must be confessed that in spite of these, which are perhaps the best determinations which have been made, the problem becomes more, and not less, formidable.

There are lines of work, however, which suggest themselves as possibly likely to throw light on the question. First, there is a striking anomaly in the atomic weight of nitrogen, determined by analysis and determined by density. Stas obtained the number 14.04 ($O = 16$), and Richards has recently confirmed his results; while Rayleigh and Leduc consistently obtained densities which, even when corrected so as to equalize the numbers of molecules in equal volumes, give the lower figure 14.002. The difference is 1 in 350; far beyond any possible experimental error. Recently, an attempt to combine the two methods has led to a mean number; but that result can hardly be taken as final. What is the reason of the discrepancy? Its discovery will surely advance knowledge materially. I would suggest the preparation of pure compounds of nitrogen, such

as salts of hydrazine, methylanine, etc., and their careful analysis; and also the accurate determination of the density and analysis of such gaseous compounds of nitrogen as nitric oxide and peroxide. I have just heard from my former student, R. W. Gray, that he has recovered Stas's number by combining 2NO with O_2; while the density of NO leads to the lower value for the atomic weight of nitrogen.[1]

The question of the atomic weight of tellurium appears to be settled, at least so far as its position with regard to the generally accepted atomic weight of iodin is concerned; recent determinations give the figures 127.5 (Gutbier), 127.6 (Pellini), and 127.9 (Köthner). But is that of iodin as accurately known? It would appear advisable to revise the determination of Stas, preparing the iodin preferably from an organic compound, such as iodoform, which can be produced in a high state of purity. The heteromorphism of selenates and tellurates, too, has recently been demonstrated; and it may be questioned whether these elements should both belong to the same group.

The rare earths still remain a puzzle. Their number is increasing yearly, and their claim to individuality admits of less and less dispute. What is to be done with them? Are they to be grouped by themselves as Brauner and Steele propose? If so, how is their connection with other elements to be explained? Recent experiments in my laboratory have convinced me that in the case of thorium, at least, ordinary tests of purity such as fine crystals, constant subliming point, etc., do not always indicate homogeneity; or else that we are sadly in want of some analytical method of sufficient accuracy. The change of thorium into thorium

[1] Note added February, 1906; Researches by Gray and by Guye have since shown that Stas's results are in error; and determinations by Richards allow the same conclusions to be drawn. The actual atomic weight cannot differ much from 14.007.

X is perhaps hardly an explanation of the divergences; yet it must be considered; but of this, anon.

To turn next to another problem closely related to the orderly arrangement of the elements,—that of valency,—but little progress can be chronicled. The suggestions which have been made are speculative, rather than based on experiment. The existence of many peroxidized substances, such as percarbonates, perborates, persulphates, and of crystalline compounds of salts with hydrogen peroxide, makes it difficult to draw any indisputable conclusions as regards valency from a consideration of oxygen compounds. Moissan's brilliant work on fluorides, however, has shown that SF_6 is capable of stable existence, and this forms a strong argument in support of the hexad character of sulphur. The tetravalency of oxygen, under befitting conditions, too, is being acknowledged, and this may be reconciled with the existence of water of crystallization, as well as of the persalts already mentioned. The adherence of ammonia to many chlorides, nitrates, etc., points to the connecting link being ascribable to the pentavalency of nitrogen; and it might be worth while investigating similar compounds with phosphoretted and arseniuretted hydrogen, especially at low temperatures.

The progress of chemical discovery, indeed, is closely connected with the invention of new methods of research, or the submitting of matter to new conditions. While Moissan led the way by elaborating the electric furnace, and thus obtained a potent agent in temperatures formerly unattainable, Spring has tried the effect of enormous pressure, and has recently found chemical action between coprous oxide and sulphur at ordinary temperature, provided the pressure be raised to 8000 atmospheres. Increase of pressure appears to lower the temperature of reaction. It has been known for long that explosions will not propagate in rarefied

gases, and that they become more violent when the reacting gases are compressed: but we are met with difficulties, such as the non-combination of hydrogen and nitrogen, even at high temperature and great pressure; yet it is possible to measure the electromotive force (0.59 volt) in a couple consisting of gaseous nitrogen and gaseous hydrogen, the electrolyte being a solution of ammonium nitrate saturated with ammonia. Chemical action between dissolved hydrogen and nitrogen undoubtedly occurs; but it is not continuous. Again we may ask, Why? The heat evolution should be great; the gain of entropy should also be high were direct combination to occur. Why does it not occur to any measurable extent? Is it because for the initial stages of any chemical reaction, the reacting molecules must be already dissociated, and those of nitrogen are not? Is that in any way connected with the abnormally low density of gaseous nitrogen? Or is it that, in order that combination shall occur, the atoms must fit each other; and that, in order that nitrogen and hydrogen atoms may fit, they must be greatly distorted? But these are speculative questions, and it is not obvious how experiments can be devised to answer them.

Many compounds are stable at low temperatures which dissociate when temperature is raised. Experiments are being made, now that liquid air is to be purchased or cheaply made, on the combinations of substances which are indifferent to each other at ordinary temperatures. Yet the research must be a restricted one, for most substances are solid at—185°, and refuse to act on each other. It is probable, however, that at low temperatures compounds could be formed in which one of the elements would possess a greater valency than that usually ascribed to it; and also that double compounds of greater complexity would prove stable. Valency, indeed, appears to be in many cases a function of temperature; exothermic compounds, as is well

known, are less stable, the higher the temperature. The sudden cooling of compounds produced at a high temperature may possibly result in forms being preserved which are unstable at ordinary temperatures. Experiments have been made in the hope of obtaining compounds of argon and helium by exposing various elements to the influence of sparks from a powerful induction coil, keeping the walls of the containing-vessel at the temperature of liquid air, in the hope that any endothermic compound which might be formed would be rapidly cooled, and would survive the interval of temperature at which decomposition would take place naturally. But these experiments have so far yielded only negative results. There is some indication, however, that such compounds are stable at 1500°. It might be hoped that a study of the behavior of the non-valent elements would have led to some conception of the nature of valency; but so far, no results bearing on the question have transpired. The condition of helium in the minerals from which it is obtainable by heat is not explained; and experiments in this direction have not furnished any positive information. It is always doubtful whether it is advisable to publish the results of negative experiments; for it is always possible that some more skilled or more fortunate investigator may succeed, where one has failed. But it may be chronicled that attempts to cause combination between the inactive gases and lithium, potassium, rubidium, and cæsium have yielded no positive results; nor do they appear to react with fluorin. Yet conditions of experiment play a leading part in causing combination, as has been well shown by Moissan with the hydrides of the alkali-metals, and by Guntz, with those of the metals of the alkaline earths. The proof that sodium hydride possesses the formula NaH, instead of the formerly accepted one, removes one difficulty in the problem of valency; and SrH_2 falls into its natural position among hydrides.

A fertile field of inorganic research lies in the investigation of structure. While the structure of organic compounds has been elucidated almost completely, that of inorganic compounds is practically undeveloped. Yet efforts have been made in this direction which appear to point a way. The nature of the silicates has been the subject of research for many years by F. W. Clarke; and the way has been opened. Much may be done by treating silicates with appropriate solvents, acid or alkaline, which differentiate between uncombined and combined silica, and this in some cases, by replacement of one metal by another, gives a clue to constitution. The complexity of the molecules of inorganic compounds, which are usually solid, forms another bar to investigation. It is clear that sulphuric acid, to choose a common instance, possesses a very complicated molecule; and the fused nitrates of sodium and potassium are not correctly represented by the simple formulæ $NaNO_3$, and KNO_3. Any theory of the structure of their derivatives must take such facts into consideration; but we appear to be getting nearer the elucidation of the molecular weights of solids. Again, the complexity of solutions of the most common salts is maintained by many investigators; for example, a solution of cobalt chloride, while it undoubtedly contains among other constituents simple molecules of $CoCl_2$, also consists of ions of a complex character, such as $(CoCl_4)''$. And what holds for cobalt chloride also undoubtedly holds for many similar compounds.

In determining the constitution of the compounds of carbon, stereo-chemistry has played a great part. The ordinary structural formulæ are now universally acknowledged to be only pictorial, if, indeed, that word is legitimate; perhaps it would be better to say that they are distorted attempts at pictures, the drawing of which is entirely free from all rules of perspective. But these formulæ may in almost every

case be made nearly true pictures of the configuration of the molecules. The benzene formula, to choose an instance which is by no means the simplest, has been shown by Collie to be imitated by a model which represents in an unstrained manner the behavior of that body on treatment with reagents. But in the domain of inorganic chemistry, little progress has been made. Some ingenious ideas of the geologist Sollas on this problem have hardly received the attention which they deserve; perhaps they may have been regarded as too speculative. On the other hand, Le Bel's and Pope's proof of the stereo-isomerism of certain compounds of nitrogen; Pope's demonstration of the tetrahedral structure of the alkyl derivatives of tin; and Smile's syntheses of stereo-isomeric sulphur compounds give us the hope that further investigation will lead to the classification of many other elements from this point of view. Indeed, the field is almost virgin soil; but it is well worth while cultivating. There is no doubt that the investigation of other organometallic compounds will result in the discovery of stereoisomerides; yet the methods of investigation capable of separating such constituents have in most cases still to be discovered.

The number of chemical isomerides among inorganic compounds is a restricted one. Werner has done much to elucidate this subject in the case of complex ammonia derivatives of metals and their salts; but there appears to be little doubt that if looked for, the same or similar phenomena would be discoverable in compounds with much simpler formulæ. The two forms of SO_3, sulphuric and hydridi are an instance in point. No doubt formation under different conditions of temperature and pressure might result in the greater stability of some forms which under our ordinary conditions are changeable and unstable. The fact that under higher pressures than are generally at our disposal dif-

ferent forms of ice have been proved to exist, and the application of the phase rule to such cases will greatly enlarge our knowledge of molecular isomerism.

The phenomena of catalysis have been extensively studied of recent years, and have obviously an important bearing on such problems. A catalytic agent is one which accelerates or retards the velocity of reaction. Without inquiring into the mechanism of catalysis, its existence may be made to influence the rate of chemical change, and to render bodies stable which under ordinary conditions are unstable. For if it is possible to accelerate a chemical change in such a way that the usually slow and possibly unrecognizable rate of isomeric change may be made apparent and measurable, a substance the existence of which could not be recognized under ordinary circumstances, owing to its infinitesimal amount, may be induced to exist in weighable quantity, if the velocity of its formation from an isomeride can be greatly accelerated by the presence of an appropriate catalytic agent. I am not aware that attempts have been made in this direction. The discovery of catalytic agents is, as a rule, the result of accident. I do not think that any guide exists which would enable us to predict that any particular substance would cause an acceleration or a retardation of any particular reaction. But catalytic agents are generally those which themselves, by their power of combining with or parting with oxygen, or some other element, cause the transfer of that element to other compounds to take place with increased or diminished velocity. It is possible, therefore, to cause ordinary reactions to take place in presence of a third body, choosing the third body with a view to its catalytic action, and to examine carefully the products of the main reaction as regards their nature and their quantity. Attempts have been made in this direction with marked success; the rate of change of hydrogen

dioxide, for example, has been fairly well studied. But what has been done for that compound may be extended indefinitely to others, and doubtless with analogous results. Indications of the existence of as yet undiscovered compounds may be derived from a study of physical, and particularly of electrical changes. There appears to be sufficient evidence of an oxide of hydrogen containing more oxygen than hydrogen dioxide, from a study of the electromotive force of a cell containing hydrogen dioxide; yet the higher oxide still awaits discovery.

The interpretation of chemical change in the light of the ionic theory may now be taken as an integral part of inorganic chemistry. The ordinary reactions of qualitative and quantitative analysis are now almost universally ascribed to the ions, not to the molecules. And the study of the properties of most ions falls into the province of the inorganic chemist. To take a familiar example: The precipitation of hydroxides by means of ammonia-solution has long led to the hypothesis that the solution contained ammonium hydroxide; and indeed, the teaching of the text-books and the labels on the bottles supported this view. But we know now that a solution of ammonia in water is a complex mixture of liquid ammonia and liquid water; of ammonium hydroxide, NH_4OH; and of ions of ammonium $(NH_4)'$, and hydroxyl $(OH)'$. Its reactions, therefore, are those of such a complex mixture. If brought into contact with a solution of some substance which will withdraw the hydroxyl ions, converting them into water, or into some non-ionized substance, they are replaced at the expense of the molecules of non-ionized ammonium hydroxide; and these, when diminished in amount, draw on the store of molecules of ammonia and water, which combine, so as to maintain equilibrium. Now the investigation of such changes must belong to the domain of inorganic chem-

istry. It is true that the methods of investigation are borrowed from the physical chemist; but the products lie in the province of the inorganic chemist. Indeed, the different departments of chemistry are so interlaced that it is impossible to pursue investigations in any one branch without borrowing methods from the others; and the inorganic chemist must be familiar with all chemistry, if he is to make notable progress in his own branch of the subject. And if the substances and processes investigated by the inorganic chemist are destined to become commercially important, it is impossible to place the manufacture on a sound commercial basis without ample knowledge of physical methods, and their application to the most economical methods of accelerating certain reactions and retarding others, so as to obtain the largest yield of the required product at the smallest cost of time, labor, and money.

I have endeavored to sketch some of the aspects of inorganic chemistry with a view to suggesting problems for solution, or at least the directions in which such problems are to be sought. But the developments of recent years have been so astonishing and so unexpected, that I should fail in my duty were I not to allude to the phenomena of radioactivity, and their bearing on the subject of my address. It is difficult to gauge the relative importance of investigations in this field; but I may be pardoned if I give a short account of what has already been done, and point out lines of investigation which appear to me likely to yield useful results.

The wonderful discovery of radium by Madame Curie, the preparation of practically pure compounds of it, and the determination of its atomic weight, are familiar to all of you. Her discovery of polonium, and Debierne's of actinium, have also attracted much attention. The recognition of the radioactivity of uranium by Becquerel, which

gave the first impulse to these discoveries, and of that of thorium by Schmidt, is also known.

These substances, however, presented at first more interest for the physicist than the chemist, on account of the extraordinary power which they all possess of emitting "rays." At first, these rays were supposed to constitute ethereal vibrations; but all the phenomena were not explicable on that supposition. Schmidt first, and Rutherford and Soddy later, found that certain so-called "rays" really consist of gases; and that while thorium emits one kind, radium emits another; and no doubt Debierne's actinium emits a third. The name "emanations" was applied by Rutherford to such radioactive bodies; he and Soddy found that those of radium and thorium could be condensed and frozen by exposure to the temperature of liquid air, and that they were not destroyed or altered in any way by treatment with agents which are able to separate all known gases from those of the argon group, namely, red-hot magnesium-lime, and it was later found that sparking with oxygen in presence of caustic potash did not affect the gaseous emanation from radium. The conclusion therefore followed that in all probability these bodies are gases of the argon group, the atomic weight of which, and consequently the density, is very high; indeed, several observers, by means of experiments on the rate of diffusion of the gas from radium, believe it to have a density of approximately 100, referred to the hydrogen standard. This conclusion has been confirmed by the mapping of the spectrum of the radium emanation, which is similar in general character to the spectra of the inactive gases, consisting of a number of well-defined, clearly cut brilliant lines, standing out from a black background. The volume of the gas produced spontaneously from a given weight of radium bromide in a given time has been measured; and it was incidentally shown that

this gas obeys Boyle's law of pressures. The amount of gas thus collected and measured, however, was very minute; the total quantity was about the forty-thousandth of a cubic centimeter.

Having noticed that those minerals which consist of compounds of uranium and thorium contain helium, Rutherford and Soddy made the suggestion that it might not be impossible that helium is the product of the spontaneous change of the emanation; and Soddy and I were able to show that this is actually the case. For, first, when a quantity of radium salt which has been prepared for some time is dissolved in water, the occluded helium is expelled, and can be recognized by means of its spectrum; further, the fresh emanation shows no helium spectrum, but after a few days the spectrum of helium begins to appear, proving that a spontaneous change is in progress; and last, as the emanation disappears its volume decreases to zero; and on heating the capillary glass tube which contained it, helium is driven out from the glass walls, into which its molecules had been imbedded in volume equal to three and a half times that of the emanation. The *a* rays, as foreshadowed by Rutherford and Soddy, consist of helium particles.

All these facts substantiate the theory, devised by Rutherford and Soddy, that the radium atom is capable of disintegration, one of the products being a gas, which itself undergoes further disintegration, forming helium as one of its products. Up till now, the sheet-anchor of the chemists have been the atom. But the atom itself appears to be complex, and to be capable of decomposition. It is true that only in the case of a very few elements, and these of high atomic weight, has this been proved. But even radium, the element which has by far the most rapid rate of disintegration, has a comparatively long life; the period of half-change of any given mass of radium is approximately 1100

years. The rate of change of the other elements is incomparably slower. This change, too, at least in the case of radium and its emanation, and presumably also in the case of other elements, is attended with an enormous loss of energy. It is easy to calculate from heat measurements (and independent and concordant measurements have been made) that one pound of emanation is capable of parting with as much energy as several hundred tons of nitroglycerine. The order of the quantity of energy evolved during the disintegration of the atom is as astonishing as the nature of the change. But the nature of the change is parallel to what would take place if an extremely complicated hydrocarbon were to disintegrate; its disruption into simpler paraffins and olefines would also be attended with loss of energy. We may therefore take it, I think, that the disintegration hypothesis of Rutherford and Soddy is the only one which will meet the case.

If radium is continually disappearing, and would totally disappear in a very few thousand years, it follows that it must be reproduced from other substances, at an equal rate. The most evident conjecture, that it is formed from uranium, has not been substantiated. Soddy has shown that salts of uranium, freed from radium, and left for a year, do not contain one ten-thousandth part of the radium that one would expect to be formed in the time. It is evident, therefore, that radium must owe its existence to the presence of some other substances. but what they are is still unascertained.

During the investigation of Rutherford and Soddy of the thorium emanation, a most interesting fact was observed, namely, that precipitation of the thorium as hydroxide by ammonia left unprecipitated a substance, which they termed thorium *X*, and which was itself highly radioactive. Its radioactive life, however, was a short one; and as it de-

cayed, it was reproduced from its parent thorium at an equal rate. Here is a case analogous to what was sought for with radium and uranium; but evidently uranium is not the only parent of radium; the operation is not one of parthenogenesis. Similar facts have been elicited for uranium by Crookes.

The *a* rays, caused by the distintegration of radium and of its emanation, are accompanied by rays of quite a different character; these are the *β* rays, identical with electrons, the mass of which has been measured by J. J. Thompson and others. These particles are projected with enormous velocity, and are capable of penetrating glass and metal screens. The power of penetration appears to be proportional to the amount of matter in the screen, estimated by its density. These electrons are not matter; but, as I shall relate, they are capable of causing profound changes in matter.

For the past year, a solution of radium bromide has been kept in three glass bulbs, each connected to a Topler pump by means of capillary tubing. To insure these bulbs against accident, each was surrounded by a small beaker; it happened that one of these beakers consisted mainly of potash glass; the other two were of soda glass. The potash-glass beaker became brown, while the two soda-glass beakers became purple. I think there is every probability that the colors are due to liberation of the metals potassium and sodium in the glass. They are contained in that very viscous liquid, glass, in the colorless ionic state; but these ions are discharged by the *β* rays, or negative electrons, and each metal imparts its own peculiar color to the glass, as has been shown by Maxwell Garnett. This phenomenon, however interesting, is not the one to which I desire to draw special attention. It must be remembered that the beakers have been exposed only to *β* rays; *a* rays have never been

in contact with them; they have never been bombarded by what is usually called matter, except by the molecules of the surrounding air. Now these colored beakers are radioactive, and *the radioactive film dissolves in water.* After careful washing, the glass was no longer radioactive. The solution contains an emanation, for on bubbling air through it, and cooling the issuing air with liquid air, part of the radioactive matter was retained in the cooled tube. This substance can be carried into an electroscope by a current of air, after the liquid air has been withdrawn, and as long as the air-current passes, the electroscope is discharged; the period of decay of this emanation, however, is very rapid, and on ceasing the current of air, the leaves of the electroscope cease to be discharged. In having such a short period of existence, this emanation resembles the one from actinium.

Owing to the recess, only a commencement has been made with the investigation of the residue left on evaporation of the aqueous solution. On evaporation, the residue is strongly active. Some mercurous nitrate was then added to the dissolved residue, and it was treated with hydrochloric acid in excess, to precipitate mercurous chloride. The greater part of the active matter was thrown down with the mercurous chloride, hence it appears to form an insoluble chloride. The mercurous chloride retained its activity unchanged in amount for ten days. The filtrate from the mercurous chloride, on evaporation, turned out to be active; and on precipitating mercuric sulphide in it, the sulphide precipitate was also active; but its activity decayed in one day. The filtrate from the mercuric sulphide gave inactive precipitates with ferric salts and ammonia, with zinc salts and ammonium sulphide, with calcium salts and ammonium carbonate; and on final evaporation, the residue was not radioactive. Hence the active matter forms

an insoluble chloride and sulphide. The precipitated mercurous chloride and mercuric sulphide were dissolved in *aqua regia,* and the solution was evaporated. The residue was dissolved in water, and left the dish inactive. But the solution gave an insoluble sulphate, when barium chloride and sulphuric acid were added to it; hence the radioactive element forms an insoluble sulphate, as well as an insoluble chloride and sulphide.

This is a sample of the experiments which have been made. It may be remarked that the above results were obtained from a mixture of the potash and soda glass: somewhat different results were obtained from the potash glass alone. These changes appear to be due to the conversion of one or more of the constituents of the glass into other bodies. Needless to say, neither of the samples of glass contained lead.

I have mentioned these experiments in detail, because I think they suggest wholly new lines of investigation. It would appear that if energy can be poured into a definite chemical matter, such as glass, it undergoes some change, and gives rise to bodies capable of being tested for; I imagine that radioactive forms of matter are produced, either identical with or allied to those at present known. And just as radium and other radioactive elements suffer degradation spontaneously, evolving energy, so I venture to think that if energy be concentrated in the molecules of ordinary forms of matter, a sort of polymerization is the result, and radioactive elements, probably elements with high atomic weight, and themselves unstable, are formed. Of course further research may greatly modify these views; but some guide is necessary, and Mr. Ternent Cook, who has helped me in these experiments, and I, suggest this hypothesis (in the words of Dr. Johnstone Stoney, an hypothesis is "a supposition which we hope may be useful") to serve as a guide for future endeavor.

In the light of such facts, speculation on the periodic arrangement of the elements is surely premature. It is open to any one to make suggestions; they are self-evident. Most of you will agree with the saying, "It is easy to prophesy after the event." I prefer to wait until prophecy becomes easy.

I must ask your indulgence for having merely selected a few out of the many possible views as regards the Problems of Inorganic Chemistry. I can only plead in excuse that my task is not an easy one; and I venture to express the hope that some light has been thrown on the shady paths which penetrate that dark region which we term the future.

PROBLEMS IN NUTRITION

BY OTTO COHNHEIM

(*Translated from the German by Prof. J. L. R. Morgan, Columbia University.*)

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THE object of the papers read here is not so much the consideration of any one restricted branch of science as it is the discussion of those broader fields which lie between and are intimately connected with several branches of science. In accord with this I propose to speak on a subject belonging primarily to the physiology of nutrition, but one which at the same time has very great politico-economic importance. To-day, as the result of the great progress which has been made in the physiology of nutrition, we can in general give a definite answer to the question as to the extent of the agreement between the actually observed dietary of an individual or group of individuals, and the conclusions obtained theoretically. At any rate to-day we can account physiologically for, and regard as physiologically necessary, a whole series of phenomena which in the past could only be accepted as empirical facts. The physiological consideration of race-dietary, on the other hand, will show how it happened that social considerations for decades have directed and restricted physiological progress. The food of man, as is well known, is composed of proteids, fats, and carbohydrates. In most food-stuffs we have all three classes of substances; only sugar and butter belong solely

to one class, the former being a carbohydrate, the latter a fat. The proteids assume a particularly important position owing to the fact that our bodies themselves are composed to a very large extent of proteinaceous material and hence can only be built up by proteids. The major portion of the proteids we absorb are derived from bread and meat. The foods richest in proteids are meat, fish, eggs, cheese, milk, etc.; in short, those foods having an animal origin. The older physiology considered the material composition of the food as the essential characteristic, although even Liebig recognized metabolism as a process of combustion, and it is the work of the Voit school which has caused the calorimetrical value of food to attain its present central position. By its combustion, the nutriment absorbed supplies the energy which is required by the human body for its various purposes. The value of a food, then, can be expressed by the amount of energy it can produce, and this value can be stated clearly and accurately in the ordinary terms of energy, *i. e.,* in units of heat, or calories. As the result of years of work by various investigators it has been found that the individual foods can be almost completely represented by their calorimetrical values. Rubner, Zuntz, and Atwater, by differing methods, have all come to the same conclusion, viz., that for purposes of heat and muscular action, *i. e.,* for its principal requirements, proteids, fats, and carbohydrates, the organism can employ vegetable and animal foods equally well. That the civilized nations of Europe and America employ bread and meat as the principal source, while the Indians and Chinese use rice exclusively, and the Esquimos fat, is not due to any difference in physiological organization, or to differing needs of the body, but simply to the more or less easy attainment of the substances, fruitfulness of the soil, and other secondary circumstances.

The law of the calorimetrical equivalency of all food-

stuffs has but one notable exception. So far as investigation has been carried out it has been found that the dietary of any man or race always contains a certain and apparently similar amount of proteinaceous material. The kind of material seemingly has little influence, but about 100 gr. of protein is found with great constancy in the daily food of the individual. In the food of a powerful man, who exerts a fairly large amount of muscular effort, Voit found 118 gr. of proteids per day, and he assumes this as a basis for the dietary of a soldier. Weaker men, doing less muscular work, require, according to Voit, a smaller quantity of proteids. For the poorly nourished, and also for those who are incapable of any intense effort, the hand-loom weavers of Zittau, the poor of Naples, and the poorest negroes of Alabama, von Rechenberg, Manfredi, and Atwater have found much lower amounts. During comparatively short laboratory experiments, Munk, Hirschfield, Kumagawa, and especially Sivén have found considerably smaller quantities. For well-nourished men, during long periods, Chittenden, only, found less proteids; otherwise, physiological investigation, as well as the experience of daily life, has shown that it is not well to consume less than 100 gr. of proteids per day. This amount, indeed, is rarely exceeded, for Chittenden has shown that even the diet of well-to-do Americans, which appears to us as the richest in proteids, scarcely ever exceeds 100 gr. of proteids a day, and the investigation of the freely-chosen fare of the most various individuals leads to the same result.

The question as to the need of the human body for 100 gr. of proteinaceous material per day has often been raised; and even to-day cannot be answered with certainty. During the last years, however, we have learned of a series of reasons which may serve to throw some light upon the subject.

That the growing organism requires proteids is self-evident, for in this way only can it obtain the materials of which it is composed. We know further, however, that the adult organism continually repairs and increases its organs and consequently also requires proteids. According to Zuntz a man increases his muscles when he does unaccustomed work (for example, when he learns a new sport) or even by increased exertion upon his usual work. Bunge attributes a considerable requirement of proteids in adults to the loss of organ-proteids in the sperm of man, and to menstruation, pregnancy, and lactation in woman. And later years have disclosed the genetic relations of many decomposition-products of the proteids with carbohydrates, with substances of the bile, and others, which are necessary, at any rate for a time, to neutralize poisons, or which are essential for the intermediate metabolism; and these relations appear to render desirable at least the presence of a copious supply of proteinaceous material.

A second reason is more difficult to grasp. Even the first metabolic experiments of Voit showed that although the proteids possess no higher nutritive (fuel) value than the carbohydrates, and a very much smaller nutritive (fuel) value than the fats, they burn very much more rapidly; and this has since been repeatedly confirmed. When the supply of proteids in the food is increased above the actual need of the body, the fats and carbohydrates are stored up, and the proteids are burned to a very much greater extent. The relations between the cells of our bodies and the substances absorbed as nutriment can best be illustrated by an analogy. For the neutralization of an alkali any acid may be employed; but when several acids of differing strength are present together, the strongest one will be partially saturated before the others even begin to react. In the same way protoplasm can supply its need with all three nutritive sub-

stances; but when all three are present at once the proteids burn first. With a large excess of the other two, however, the action of their masses becomes evident, exactly as in the illustration with the acids, and they protect the proteids from combustion. In the absence of fats and carbohydrates, the body readily goes into such a state that its own proteids are attacked, *i. e.,* it consumes itself. This is probably the most important reason why a definite minimum amount of proteids is essential in a small total amount of nutriment. That further differences exist among the individual organs themselves is still to be proven, but at present it appears quite probable.

A third ground has been disclosed during the past few years by the work of the great Russian investigator, Pawlow. We knew from this that the nervous connection of the digestive system with the sense organs of the head determines the enjoyment of the food, and hence regulates the choice of that. We know further from Pawlow, Weinland, and Starling that this connection is not fixed once for all, but varies according to the needs of the time. When any such relation is observed we must always conclude that it is adapted to an end, for otherwise it would have disappeared within a short time. Pure proteids are tasteless and odorless, and also fail to act upon the sensitive nerves of the stomach and intestine; in all natural foods, on the other hand, the proteids are always associated with the pleasant-tasting constituents of nutriment, and those which stimulate digestion. For us, just as for the carnivorous animals investigated by Pawlow, the substances richest in proteids are always the most pleasant to the taste, and those which arouse the appetite the most. The foods which are poorer in proteids, as rice and potatoes, stimulate the digestion less and consequently are more difficultly digestible. A food-stuff free from proteids has already been

shown in animal experiments to be impossible as a diet, and even in experiments with substances which are poor in proteids Sivén and Röhl encountered insurmountable difficulties owing to the tastelessness of the material.

Even though we do not as yet know all the reasons, it is at any rate obvious that, for long periods of time and for normal nutrition, Voit has discovered the correct condition, viz., that an amount of proteids equal to 100 gr. per day is essential, or at any rate can be designated as desirable.

Since in consequence of the special internal organization of the human body, and because it is the minimum amount used by all men, this amount of proteids is independent of the form of nourishment absorbed, and independent of the habits of life. Even as early as 1860 and 1866 Voit showed that the protein consumption of those doing hard work is not greater than of those who do none; and this result has been confirmed many times. The American physiologist, Atwater, has made an especial study of this question, using his respiration calorimeter. As the average of numerous experiments, carried out with the greatest exactness, he found that the subject of experiment, whether resting or working, decomposed the same amount of proteids, even when the production of calories by the work rose to double or more. Indeed, the decomposition of proteids can even be decreased by muscular activity, for the larger total of nutriment consumed prevents the decomposition of the proteids of the body.

The total amount of nutriment of a man is almost exclusively determined by the muscular work he performs. The mental work has nothing to do with nutrition; whether the brain is used intensely, or whether it is retained as inactive as is possible, as far as we know to-day, does not seem to affect the requirement of energy by the body, nor its requirement of food. The amount of energy required

by the individual to sustain his bodily temperature differs but slightly, for the differences in external temperature are nearly compensated by the wonderfully acting heat regulation of our bodies, and the artificial heat regulation by our clothes and dwellings. The influence of muscular activity is very much greater. A man resting quietly in a warm room requires from 1500-1700 calories per day; while one working in the laboratory, or sitting, produces from 2100-2400 calories. For light hand labor this is increased to 2800, while for laborers, Liebig and others have observed from 4000-6000 calories, and Atwater and Wood found up to 8000 calories for the lumbermen of Maine. As the average of all his experiments, Atwater found 2270 calories for quiescent, and 4550 calories for hard-working people, *i. e.,* exactly double the value.

Although the total number of calories varies according to the work, the amount of proteids for all men remains approximately equal, and from this we can draw an important conclusion. The food of those not doing physical work must be relatively richer in proteids, for an equal absolute amount of proteids must be contained in a smaller total amount of food. The foods richest in proteids are meat and the other products of the animal kingdom, and it is evident that the diet must be the richer in meat, the less physical work done by the person. An illustrative example will make this quite clear. A laborer does hard physical work, and consequently requires a diet which produces 5000 calories per day. Consuming only bread, potatoes, and other vegetable products, he would obtain 100 gr. of proteids and even more without trouble. Let us assume that he moves to a city and changes his occupation, living a sedentary life. For this he would require but 2500 calories, and retaining the quality of his dietary he would have to do one of two things. Either he must eat the

previous quantity, which would be impossible for any length of time, for the body could not use such an excessive amount, or he must decrease it to one half, whereby he would obtain the requisite number of calories, but with them only 50 gr. of proteids. To nourish himself properly, then, he will have to decrease his allowance of food to one half, and add to it 50 gr. of proteids, *i. e.,* about 250 gr. of meat. This example, of course, is extreme, and will not often be observed with such distinctness. The principle, however, is always to be observed. The food of those belonging to the well-to-do classes, *i. e.,* of those who do no hard physical work, in all countries contains the most meat. This, however, is no luxury, but is based upon physiological grounds. Comparing different countries, or different classes in the same country, we always obtain the following result. To the degree that pure hand labor is replaced by the work of the head, and that of overseeing machines, to that same degree is the consumption of meat increased. This is shown most obviously, however, by the comparison of the country population with that of the city. The modern mill-hand lives, it is true, by the work of his hands, but that work is quite different from that done by the farm laborer. The overseeing and directing of the complicated machines, as every other form of skilled labor, requires attention, intelligence and dexterity, but does not require the muscular exertion necessary for mowing, threshing, and the felling of trees. With the difference in activity there must also be a difference in the quantity and kind of food. The people in a city in general eat less in total amount, but this food is qualitatively different, *i. e.,* must consist of substances relatively rich in proteids, as meat and other animal products.

From the politico-economical, as well as from the medical, point of view the smaller amount of food consumed by the

mill-hand, as compared to that of the farm laborer, is regarded as a sign of degeneration. This is obviously untrue, for there is no general standard of nutrition which is applicable, or even desirable, for all men. The nutriment, with respect to quantity, is dependent solely upon the amount of muscular work done. On the other hand, the increased consumption of meat, eggs, and other foods, agreeable to the taste because rich in proteids, has been attributed to the greediness of the urban population. Nothing could be more false. It is just for this large class that the enjoyment of meat and other foods rich in proteids is a physiological postulate; and for the other large class making up the urban population, merchants, officials, clerks, etc., this is true in even a more striking degree, for the physical work necessary in such occupations is still smaller in amount, and their food must consequently be even richer in proteids.

It is not for me to draw further conclusions from the physiological principle that the food of the urban population should contain less vegetable and more animal substance. I must rather consider the influence of these relations upon physiology itself. The classes not doing severe physical work are the higher and better-to-do, and, since they are great meat-eaters, it is but too easy to conclude that in general meat-eaters are the most valuable and best. This opinion is very rife in lay circles, and even physiology has not long been free from it. The great Liebig, the founder of the doctrine of scientific nutrition, held that meat is the only active form of food (for muscular activity), and ascribed to it a very high nutritive value. Liebig's theory was disproved 44 years ago by Voit and soon afterward by Fick and Wislicenius. But even to-day there are physiologists who hold fast to the Liebig doctrine, and indeed the relics of it are still to be found everywhere

in physiology and medicine. The tenacity of life of this old error would be difficult to explain, were it not apparently supported by the daily experience that the well-to-do eat meat, eggs, etc., while the day laborers satisfy themselves with bread and potatoes.

From the difference in the diet of those who do severe muscular work and those who do none, there is a further conclusion to be drawn. The only indigestible constituent of human food is cellulose. Cellulose, being contained only in vegetable food, forms but a small constituent of the diet of the man doing little physical work. According to von Knieriem cellulose is of great importance in the process of digestion, for as indigestible, solid substance it stimulates the activity of the intestine. While carnivorous animals, with their short muscular intestine, do not require it, graminivorous animals, with their long, coiled, weak intestine, cannot do without it for any length of time. Man, in the organization of his digestive apparatus, stands midway between these two extremes, and while cellulose is not absolutely essential to him, its absence sometimes causes a motoric atrophy of the intestine which results in chronic constipation and its consequences. The connection between constipation and sedentary occupations has long been recognized, but people have been too prone to attempt to explain it mechanically, whereas the connecting link in reality is the dietary of the man leading the sedentary life. In his daily life such a man does but little physical work, and consequently in general eats little, and especially little of the vegetable food poor in proteids but rich in cellulose. We hear nothing of digestive troubles of the people living in the country, while city people, especially the well-to-do, suffer severely from them. In England and America, judging from the wide advertisement of purgatives, the trouble is much more common than in Germany; but in both lands

the rye bread, which is comparatively rich in cellulose, is replaced by fine wheat bread, which is much poorer in cellulose, and the substitution of animal products for bread is also more common than with us.

The vegetarians have been agreed on this point for a long time. They observed how many digestive and other troubles are common to the dwellers in cities (*i. e.*, where few live like the vegetarian peasant), and all without knowledge of the physiological grounds held up the peasant as the ideal for the citizen. But what is correct for the peasant, who must produce 4000-5000 calories, is not correct for those who require but 2300 calories or less. He obtains, then, as explained above, too little of proteids, or aids himself by his fondness for the vegetables rich in proteids, but at the same time poor in cellulose, and hence fails utterly to attain his end.

It would be more correct if the transition to the vegetable diet is combined with a treatment which will increase the need of substance, as has been done from non-scienitfic sides and without knowledge of the physiological principle.

The only rational cure for the disturbances which can ultimately be traced to the lack of muscular activity is to devote one's self to this muscular work outside of one's daily occupation, as is possible by aid of the various sports. It is no accident, but rather a necessary physiological phenomenon, that the need of active sports has always developed wherever there is a class of society made up of those doing no intense physical work. Indeed, we can readily follow this in history; when the citizens of the Greek cities devoted themselves to athletic sports, when the knights of the Middle Ages jousted, there was always an aristocracy who did no manual labor. The home of our modern sports is England, the oldest industrial country. In Germany the first steps in the direction of sports were made at the uni-

versities, where thousands of young men did mental work. The scope of the sport of to-day is very much broader, however, for its followers include merchants and the workers in the various industries. Sports lead directly to a change in the food requirements of the individual; every bicyclist, every mountain climber knows that on his trips he can eat things which do not appeal to him at all when at home. I must be content here to restrict myself to these indications disclosing the scientific principles of this subject, which is apparently so far removed from our point of departure. By unwearying work the physiology of nutrition has established a scientific experimental foundation upon which other sciences may now build.

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THE LIGHT OF THE STARS

BY EDWARD CHARLES PICKERING

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IF an intelligent observer should see the stars for the first time, two of their properties would impress him as subjects for careful study,—first, the irrelative positions, and secondly, the irrelative brightness. From the first of these has arisen the astronomy of position, or astrometry. This is sometimes called the Old Astronomy, since until within the last twenty years the astronomers of the world, with few exceptions, devoted their attention almost entirely to it. To the measure of the light should be added the study of the color of the stars (still in its infancy), and the study of their composition, by means of the spectroscope. In this way a young giant has been reared, which has almost dwarfed its older brothers. The science of astrophysics or the New Astronomy, has thus been developed, which during the last few years has rejuvenated the science and given to it, by its brilliant discoveries, a public interest which could not otherwise have been awakened. The application to stellar astronomy of the daguerrotype in 1850, of the photograph in 1857, and of the dry plate in 1882, has opened new fields in almost every department of this science. In some, as in stellar spectroscopy, it has almost completely replaced visual observations.

One department of the New Astronomy, the relative brightness of the stars, is as old as, or older than, the Old Astronomy. An astronomer even now might do useful work in this department without any instruments whatever. Hipparchus is known to have made a catalogue of the stars about 150 B. C. Ptolemy, in 138 A. D., issued that great work, the *Almagest,* which for fourteen hundred years constituted the principal and almost the sole authority in astronomy. It contained a catalogue of 1028 stars, perhaps based on that of Hipparchus. Ptolemy used a scale of stellar magnitudes which has continued in use to the present day. He called the brighest stars in the sky the first magnitude, the faintest visible to the naked eye, the sixth. More strictly, he used the first six letters of the Greek alphabet for this purpose. But he went a step further, and subdivided these classes. If a star seemed bright for its class, he added the letter μ (mu), standing for μεῖζων (meizon), large or bright, if the star was faint he added ε (epsilon), standing for ἐλάσσων (elasson), small or faint. These estimates were presumably carefully made, and if we had them now, they would be of the greatest value in determining the secular changes, if any, in the light of the stars. The earliest copy we have of the *Almagest* is no. 2389 of the collection in the Bibliothèque Nationale of Paris. It is a beautiful manuscript, written in the uncial characters of the ninth century. A few years ago it could be seen by any one in one of the show-cases of the library. There are many later manuscripts and printed editions which have been compared by various students. The errors in these various copies are so numerous that there is an uncertainty in the position, magnitude, or identification of about two thirds of the stars. A most important revision was made by the Persian astronomer, Abd-al-rahman al-Sufi, who re-observed Ptolemy's stars, A. D. 964, and noted

the cases in which he found a difference. The careful study and translation of this work from Arabic into French by Schjellerup has rendered it readily accessible to modern readers.

No important addition to our knowledge of the light of the stars was made until the time of Sir William Herschel, the greatest of modern observers. He found that when two stars were nearly equal, the difference could be estimated very accurately. He designated these intervals by points of punctuation, a period denoting equality, a comma a very small interval, and a dash a larger interval. In 1796 to 1799, he published in the *Philosophical Transactions* four catalogues covering two thirds of the portion of the sky visible in England. Nearly a century later, it was my great good fortune, when visiting his grandson, to discover in the family library the two catalogues required to complete this work, and which had not been known to exist. These two catalogues are still unpublished. Meanwhile, little or no use had been made of the four published catalogues which, while comparing one star with another, furnished no means of reducing all to one system of magnitudes. The Harvard measures permitted me to do this for all six catalogues, and thus enabled me to publish magnitudes for 2785 stars observed a century ago, with an accuracy nearly comparable with the best work of the present time. For nearly half a century no great advance was made, and no astronomer was wise enough to see how valuable a work he could do by merely repeating the observations of Herschel. Had this work been extended to the southern stars, and repeated every ten years, our knowledge of the constancy of the light of the stars would have been greatly increased. In 1844, Argelander proposed, in studying variable stars, to estimate small intervals, modifying the method of Herschel by using numbers instead of points of punctuation, and thus

developed the method known by his name. This is now the best method of determining the light of the stars, when only the naked eye or a telescope is available, and much valuable work might be done by applying it to the fainter stars, and especially to clusters.

Meanwhile photometric measures of the stars according to various methods had been undertaken. In 1856, Pogson showed that the scale of magnitudes of Ptolemy, which is still in use, could be nearly represented by assuming the unit to be the constant ratio, 2.512, whose logarithm is 0.4. This has been generally adopted as the basis of the standard photometric scale. The photometer devised by Zöllner has been more widely used than any other. In this instrument, an artificial star is reduced any desired amount, by polarized light, until it appears to equal the real star, both being seen side by side in the telescope. Work with this instrument has attained its greatest perfection at the Potsdam Observatory, where measures of the light of the northern stars, whose magnitude is 7.5 and brighter, have been in progress since 1886. The resulting magnitudes have been published for 12,046 stars, included in declination between —2° and +60°. The accidental errors are extremely small, but as the results of different catalogues differ systematically from one another, we cannot be sure which is right and what is the real accuracy attained in each case. In 1885, the *Uranometria Oxoniensis* was published. It gives the magnitudes of 2784 northern stars, north of declination —10°. This work is a remarkable one, especially as its author, Professor Prichard, began his astronomical career at the age of sixty-three. The method he employed was that of reducing the light of the stars by means of a wedge of shade glass until they became invisible, and then determining the brightness from the position of the wedge. A careful and laborious investi-

gation, extending over many years, has been carried on by Mr. H. M. Parkhurst, using a modification of this method.

For several years before the Oxford and Potsdam measures described above were undertaken, photometric observations were in progress at Harvard. In 1877, a large number of comparisons of adjacent stars were made with a polarizing photometer. Two images of each star were formed with a double-image prism, and the relative brightness was varied by turning a Nicol prism until the ordinary image of one star appeared equal to the extraordinary image of the other. Several important sources of error were detected, which, once known, were easily eliminated. A bright star will greatly affect the apparent brightness of an adjacent faint one, the error often exceeding a magnitude. Systematic errors amounting to several tenths of a magnitude depend upon the relative positions of the images compared. They are perhaps due to the varying sensitiveness of the different parts of the retina. This photometer has many important advantages. However bad the images may be, they are always exactly alike, and may, therefore, be compared with accuracy. Both stars are affected equally by passing clouds, so that this photometer may be used whenever the stars are visible, and at times when other photometric work is impossible. The diminution in light also follows a simple geometrical law, and is readily computed with great accuracy. There is no unknown constant to be determined, as in the Pritchard, and nearly all other photometers. The principal objections to this instrument are, first, that stars cannot be compared unless they are near together, and, secondly, that faint stars cannot be measured, since one half of the light is lost by polarization. The principal uses so far made of this form of photometer are in comparing the components of double stars, and in a long series of observations of the eclipses of Jupiter's

satellites, which now extends over a quarter of a century and includes 768 eclipses. Instead of observing the time of disappearance, a series of measurements is made, which gives a light-curve for each eclipse. Much important work might yet be done with this form of photometer, in measuring the components of doubles and of clusters, and determining the light-curves of variables which have a moderately bright star near them.

An important improvement was made in this form of photometer in 1892, by which stars as much as half a degree apart could be compared. The cones of light of two such stars are brought together by achromatic prisms, so that they can be compared as in the preceding instrument. As there is no part of the sky in which a suitable comparison star cannot be found within this distance, any star may be measured with this instrument. In the hands of Professor Wendell this photometer has given results of remarkable precision. The average deviation of the result of a set of sixteen settings is about three hundredths of a magnitude. Light-curves of variables can therefore be determined with great precision, and suspected variables can be divided into those that are certainly variable, and those whose changes are probably less than a tenth of a magnitude.

Another change in this instrument produced the meridian photometer. Instead of using the two cones from one object-glass, two object-glasses were used, mirrors being placed in front of each. In this way, stars however distant can be compared. In theory, this instrument leaves but little to be desired. Almost every source of error that can be suggested can be eliminated by proper reversions. As constructed, the telescope is placed horizontally, pointing east or west. One mirror reflects a star near the pole into the field, the other, a star upon the meridian. A slight mo-

tion of the mirror permits stars to be observed for several minutes before or after culmination. The first meridian photometer had objectives of only two inches aperture. With this instrument, 94,476 measures were made of 4260 stars, during the years 1879 to 1882. All stars were included of the sixth magnitude and brighter, and north of declination —30°. The second instrument had objectives of four inches aperture, and permitted stars as faint as the tenth magnitude to be measured. With this instrument, during the year 1882 to 1888, 267,092 measures were made of 20,982 stars, including all the catalogue stars and all the stars of the ninth magnitude and brighter, in zones twenty minutes wide, and at intervals of five degrees, from the north pole to declination —20°. In 1889, the instrument was sent to South America, where 98,744 measures were made of 7922 southern stars, extending the two preceding researches to the south pole. On the return of the instrument to Cambridge 473,216 measures were made of 29,587 stars, including all those of the magnitude 7.5 and brighter, north of declination —30°. This work occupied the years 1891 to 1898. The instrument was again sent to Peru in 1899, and 50,816 measures were made of 5332 stars, including all those of the seventh magnitude and brighter, south of declination —30°. The latest research has been the measurement of a series of stars of about the fifth magnitude, one in each of a series of regions ten degrees square. Each of these stars is measured with the greatest care on ten nights. This work has been completed and published for stars north of declination —30°, 59,428 measures having been made of 839 stars. In this count, numerous other stars have been included. Similar measures are now in progress of the southern stars, this being the third time the meridian photometer has been sent to South America. The total number of measurements ex-

ceeds a million, and the number of stars is about sixty thousand. About sixty stars can be indentified with care, and each measured four times with this instrument in an hour. The probable error of a set of four settings is ±0.08.

The principal objection to the instrument just described is the great loss of light. To measure very faint stars, another type of photometer has been devised. A 12-inch telescope has been mounted horizontally, like the meridian photometer, and an artificial star reflected into the field. The light of this star is reduced by a wedge of shade glass, until it appears equal to the star to be measured. Four hundred thousand measures have been made with this instrument during the last five years. The principal research has been the measurement of all the stars in the Bonn *Durchmusterung,* which are contained in zones ten minutes wide and at intervals of five degrees, from the north pole to declination —20°. Large numbers of stars of the tenth and eleventh magnitudes are thus furnished as standards of light. As the light of the object observed is unobstructed, any start however faint, if visible in the telescope, may be measured. Accordingly, many stars of the twelfth and thirteenth magnitudes have been selected and measured, thus furnishing faint standards. Sequences of standard stars have been selected from coarse clusters, thus permitting estimates or measures of these bodies to be reduced to a uniform photometric scale. An investigation of great value has been carried out successfully at the Georgetown College Observatory, by the Rev. J. G. Hagen, S. J. All the stars of the thirteenth magnitude and brighter have been catalogued and chartered, in a series of regions, each one degree square, surrounding variable stars of long period. Besides measuring the positions, he has determined the relative brightness of these stars. A sequence has then been selected from each of these regions, and measured at

Harvard with the 12-inch meridian photometer, thus permitting all to be reduced to a uniform scale. As the photometer was first constructed, stars brighter than the seventh magnitude could not be measured, since they were brighter than the artificial star, and could not be rendered equal to it. This difficulty was remedied by inserting a series of shades, the densest of which reduced the light by ten magnitudes. By this method, the range of the photometer may be increased indefinitely. Sirius and stars of the twelfth magnitude have been satisfactorily measured in succession. A further modification of the instrument permitted surfaces to be compared. The light of the sky at night and in the daytime, during twilight, at different distances from the moon, and different portions of the disk of the latter, have thus been compared. Measures extending over seventeen magnitudes, with an average deviation of about three hundredths of a magnitude, were obtained in this way. One light was thus compared with another six million times as bright as itself. A slight modification would permit the intrinsic brightness of the different portions of the sun's disk to be compared with that of the faintest nebulæ visible. By these instruments, the scale of photometric magnitudes has been carried as far as the thirteenth magnitude. To provide standards for fainter stars, a small appropriation was made by the Rumford Committee of the American Academy. Coöperation was secured among the Directors of the Yerkes, Lick, McCormick, Halsted, and Harvard Observatories. Similar photometers were constructed for all, in which an artificial star was reduced any desired amount by a photographic wedge. Telescopes of 40, 36, 26, 23, and 15 inches aperature, including the two largest refractors in the world, were thus used in the same way on the same research. The standards have all been selected, and nearly all of the measurements have been made. This

furnishes a striking illustration of the advantages of coöperation, and combined organization. When these observations are reduced, we shall have standard of magnitude according to a uniform scale for all stars from the brightest to the faintest visible in the largest telescopes at present in use. The 60-inch reflector of the late A. A. Common has recently been secured by the Harvard Observatory. It is hoped that still fainter stars may be measured with this instrument.

We have as yet only considered the total light of a star, so far as it affects the eye. But this light consists of rays of many different wave-lengths. In red stars, one color predominates, in blue, another. The true method is to compare the light of a given wave-length in different stars, and then to determine the relative intensity of the rays of different wave-lengths in different stars, or at least in stars whose spectra are of different types. This is the only true method, and fortunately spectrum photography permits it to be done. The Draper Catalogue gives the class of spectrum of 10,351 stars, and the relative brightness of the light whose wave-length is 430 is determined for each. In 1891, measures were published of the relative light of rays of various wave-lengths, for a number of stars whose spectra were of the first, second, and third types.

A much simpler but less satisfactory method is to measure the total light in a photographic image. As in the case of eye-photometry, this method is open to the objection that rays of different colors are combined. Blue stars will appear relatively brighter, and red stars relatively fainter, in the photograph than to the eye. This, however, is an advantage rather than an objection, since it appears to furnish the best practical measure of the color of the stars. Relative photographic magnitudes can be obtained in a variety of ways, and the real difficulty is to reduce them

to an absolute scale of magnitudes. But for this, photographic might supersede photometric magnitudes. In other respects, photography possesses all the advantages for this work that it has for other purposes, and many photometric problems are within the reach of photography, which seem hopeless by visual methods. In 1857, Professor George P. Bond, the father of stellar photography, showed that the relative light of the stars could be determined from the diameter of their photographic images. This is the method that has been generally adopted elsewhere in determining photographic magnitudes, although with results that are far from satisfactory. It is singular that although this method originated at Harvard, it is almost the only one not in use here, while a great variety of other methods have been applied to many thousands of stars, during the last eighteen years. Relative measures are obtained very satisfactorily by applying the Herschel-Argelander method to photographic images, and if these could be reduced to absolute magnitudes, it would leave but little to be desired. In the attempt to determine absolute magnitudes a variety of methods has been employed. The simplest is to form a scale of photographing a series of images, using different exposures. The image of any star may be compared directly with such a scale. To avoid the uncertain correction due to the time of exposure, different apertures may be used instead of different exposures. Another method is to attach a small prism to the objective. The image of every bright star is then accompanied by a second image a few minutes of arc distant from it, and fainter by a constant amount, as five magnitudes. Trails may be measured more accurately than circular images, and trails of stars near the pole have varying velocities, which may then be compared with one another by means of a scale. Again, images out of focus may be compared with great accuracy and rapidity

by means of a photographic wedge. These comparisons promise to furnish excellent magnitudes, if they can only be reduced to the photometric scale. A catalogue giving the photographic magnitudes of 1131 stars within two degrees of the equator, and determined from their trails, was published in 1889. Great care was taken to eliminate errors due to right ascension, so that standards in remote portions of the sky are comparable. A similar work on polar stars at upper and lower culmination determined the photographic absorption of the atmosphere, which is nearly twice as great as the visual absorption. A catalogue of forty thousand stars of the tenth magnitude, one in each square degree, has been undertaken, and the measures are nearly complete for the portion of the sky extending from the equator to declination +30°. These stars are compared, by means of a scale, with the prismatic companions of adjacent bright stars. Two measures have been made of images out of focus of 8489 stars, including all of those north of declination —20°, and brighter than the seventh magnitude. This work is being continued to the south pole. The most important completed catalogue of photographic magnitudes is the *Cape Photographic Durchmusterung,* the monumental work of Gill and Kapteyn. 454,875 stars south of declination —19° are included in this work. Unfortunately, the difficulty mentioned above, of reducing the magnitudes to an absolute system, has not been wholly overcome, but the work is published in a form which will permit this to be done later, if a method of reduction can be discovered. The extension of this great work to the north pole is one of the greatest needs of astronomy at the present time.

The map and catalogue of the Astrophotographic Congress, the most extensive research ever undertaken by astronomers, will not be discussed here, as it will doubt-

less be described by others better able than I to explain its merits. If completed, and if the difficulty of reducing the measures of brightness to a standard scale can be overcome, it will furnish the photographic magnitudes, as well as the positions, of two million stars. Time does not permit the consideration here of certain other investigations of photographic magnitudes, such as those made at Groningen. They generally relate to a comparatively small number of stars.

The suggestion that the intensity of a photographic star-image be measured by the amount of heat it cuts off from a thermopile, deserves careful study. It should give a great increase in precision, and would obviate dependence on that tool of many defects, the human eye. No use seems to have been made, so far, of this method.

The next question to be considered is, what use should be made of these various measures of the light of the stars? The most obvious application of them is to variable stars. While the greater portion of the stars undergo no changes in light that are perceptible, several hundred have been found whose light changes. A natural classification seems to be that proposed by the writer in 1880. A few stars appear suddenly, and are called new stars, or novæ. They form Class I. Class II consists of stars which vary by a large amount during periods of several months. They are known as variable stars of long period. Class III contains stars whose variations are small and irregular. Class IV contains the variable stars of short period, and Class V the Algol variables, which are usually of full brightness, but at regular intervals grow faint, owing to the interposition of a dark companion. Twenty years ago, when photography was first applied to the discovery of variable stars, only about two hundred and fifty of these objects were known. Since then, three remarkable discoveries have been

made, by means of which their number has been greatly increased. The first was by Mrs. Fleming, who, in studying the photographs of the Henry Draper Memorial, found that the stars of the third type, in which the hydrogen lines are bright, are variables of long period. From this property she has discovered 128 new variables, and has also shown how they may be classified from their spectra. The differences between the first, second, and third types of spectra are not so great as those between the spectra of different variables of long period. The second discovery is that of Professor Bailey, who found that certain globular clusters contain large numbers of variable stars of short period. He has discovered 509 new variables, 396 of them in four clusters. The third discovery, made by Professor Wolf of Heidelberg, that variables occur in large nebulæ, has led to his discovery of 65 variables. By similar work, Miss Leavitt has found 295 new variables. The total number of variable stars discovered by photography during the last fifteen years is probably five times the entire number found visually up to the present time. Hundreds of thousands of photometric measures will be required to determine the light-curves, periods, and laws regulating the changes these objects undergo.

A far more comprehensive problem, and perhaps the greatest in astronomy, is that of the distribution of the stars, and the constitution of the stellar universe. No one can look at the heavens, and see such clusters as the Pleiades, Hyades, and Coma Berenices, without being convinced that the distribution is not due to chance. This view is strengthened by the clusters and doubles seen in even a small telescope. We also see at once that the stars must be of different sizes, and that the faint stars are not necessarily the most distant. If the number of stars were infinite, and distributed acording to the laws of chance throughout in-

finite and empty space, the background of the sky would be as bright as the surface of the sun. This is far from being the case. While we can thus draw general conclusions, but little definite information can be obtained, without accurate quantitative measures, and this is one of the greatest objects of stellar photometry. If we consider two spheres, with the sun as the common centre, and one having ten times the radius of the other, the volume of the first will be one thousand times as great as that of the second. It will, therefore, contain a thousand times as many stars. But the most distant stars in the first sphere would be ten times as far off as those in the second sphere, and accordingly if equally bright would appear to have only one one-hundredth part of the apparent brightness. Expressed in stellar magnitudes, they would be five magnitudes fainter. In reality, the total number of stars of the fifth magnitude and brighter is about 1500, of the tenth magnitude, 373,000, instead of 1,500,000, as we should expect. An absorbing medium in space, which would dim the light of the more distant stars, is a possible explanation, but this hypothesis does not agree with the actual figures. An examination of the number of adjacent stars shows that it is far in excess of what would be expected if the stars were distributed by chance. Of the three thousand double stars in the Mensuræ Micrometricæ, the number of stars optically double, or of those which happen to be in line, according to the theory of probabilities, is only about forty. This fact should be recognized in any conclusions regarding the motions of the fixed stars, based upon measures of their position with regard to adjacent bright stars.

We have here neglected all conclusions based upon the difference in composition of different stars. Photographs of their spectra furnish the material for studying this problem in detail. About half of the stars have spectra in

which the broad hydrogen lines are the distinguishing feature. They are of the first type, and belong to Class A of the classification of the Henry Draper Memorial. The Milky Way consists so completely of such stars, that if they were removed, it would not be visible. The Orion stars, forming Class B, a subdivision of the first type in which the lines of helium are present, are still more markedly concentrated in the Milky Way. A large part of the other stars, forming one third of the whole, have spectra closely resembling that of the sun. They are of the second type, and form classes G and K. These stars are distributed nearly uniformly in all parts of the sky. Class M, the third type, follows the same law. Class F, whose spectrum is intermediate between classes A and G, follows the same law of distribution as classes G and K, but differs from them, if at all, in the opposite direction from Class A. There therefore seem to be actually fewer of these stars in the Milky Way than outside of it. One class of stars, the fifth type, Class O, has a very remarkable spectrum and distribution. A large part of the light is monochromatic. Of the ninety-six stars of this type so far discovered, twenty-one are in the large Magellanic Cloud, one in the Small Magellanic Cloud, and the remainder follow the central line of the Milky Way so closely, that the average distance from it is only two degrees. All of these stars, with the exception of sixteen, have been found by means of the Henry Draper Memorial.

It will be seen from the above discussion, that stellar photometry in its broadest sense furnishes the means of attacking, and perhaps of solving, the greatest problem presented to the mind of man, the structure and constitution of the stellar universe, of which the solar system itself is but a minute and insignificant molecule.

THE DEVELOPMENT OF CELESTIAL MECHANICS DURING THE NINETEENTH CENTURY

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The development of celestial mechanics during the nineteenth century is such a comprehensive theme that a fundamental treatment of it within the limits of an address of half an hour or so cannot be thought of. I am therefore limited to the presentation of the principal phases of the subject, and of course in doing so, by reason of the necessary arbitrariness of choice, may not meet the approval of this distinguished assembly.

I will first consider the development of celestial mechanics in so far as it concerns the motions of the planets.

The nineteenth century received a great inheritance from the eighteenth. With the five undying names of Euler, Clairaut, d'Alembert, Lagrange, and Laplace are linked theoretical discoveries which upon the basis of Newton's law explained all the motions of the planets, satellites, and comets in so far as they were furnished by the observations of that time. Was this inheritance so utilized during the past century, that at the end of the same the results of

observation may be considered explained by theory? The following discussion will give the answer.

Laplace's *Mécanique Céleste* gives as it were a summing-up of the development of celestial mechanics during the eighteenth century and furnishes at the same time the starting-point for the researches of the nineteenth. We recapitulate, therefore, some of the principal points of the same in order that the discussion may be more easily understood. The coördinates and the elements of the planets and satellites were expressed in series containing: (1) periodic terms (sines and cosines of multiples of the mean and true longitudes); (2) non-periodic terms involving powers of the time, *i. e.*, the so-called secular terms; (3) semi-secular terms, that is, products of the time or the angle into sine and cosine functions; the development being made in powers of the eccentricities and inclinations considered as small quantities. The appearance of the time or the angle explicitly, outside the sine and cosine functions, was considered, at least in part, both by Laplace and Lagrange as the result of incomplete operations. But on the other hand, from the standpoint of astronomy, it was considered entirely useless to complicate the expressions by introducing trigonometrical series in place of angles. The constants of integration, *i. e.*, the elements, were determined numerically for each of the planets then known, and the numerical values of the coefficients of the individual terms of the series were derived therefrom. It was then sufficient in most cases to consider only the lowest powers of the eccentricities in order to obtain the places of the planets with an accuracy corresponding with that of the observations. As a result astronomers were enabled to explain all the observed inequalities; for example, the great inequality in the motions of Jupiter and Saturn, the inequality in the motion of Jupiter's satellites discovered by

Wargentin, etc. With the aid of his epoch-making theory of the variation of constants, Lagrange proved the famous theory that the expression for the major axis contains only periodic terms, when powers of the mass higher than the first are neglected. Both Lagrange and Laplace had found that in the first approximation the eccentricities and inclinations may be considered as long-period functions of the time. Through the researches of Laplace the theory of the moon, the motion of the earth about its centre of gravity, and the theory of the figures of the planets, were so developed as completely to satisfy the corresponding observations.

These brief statements of some of the principal points may be sufficient.

The nineteenth century was introduced by two researches of Poisson of remarkable value to celestial mechanics. One was the extension of Lagrange's theory in regard to the major axis to the second power of the mass; the other and far more important was his classic theory of the motion of the earth about its centre of gravity, which was built up by the aid of the method of the variation of constants. There was then for a time a pause in the development of the theories of the major planets.

The transition to the new century was, however, marked by the beginning of a series of discoveries which are to-day still being carried forward, and which in one direction have exerted an important influence in the development of celestial mechanics. I refer to the discovery of the small planets. Laying aside the numerical calculation of special perturbations, which has been developed to a high degree of refinement, the perturbation problem will be considered here in the sense in which it was brought over from the preceding century. The interpolation formulæ which the special perturbations offer can permit only an extremely in-

complete insight into the nature of the motion. To be sure, the general perturbation formulæ in the form given by Laplace are also to be considered merely as interpolation formulæ, since they hold for only a limited time, practically nothing being known in regard to the absolute convergence of the series of secular terms. We shall first of all follow the important investigations which have been made for the purpose of representing the motion of a small planet by means of general perturbation formulæ in the sense spoken of.

In addition to the *Theoria Motus Corporum Coelestium,* which, for apparent reasons, does not here come under consideration, Gauss busied himself with the theory of the minor planets, by making extended investigations on the perturbations of Pallas. He did not bring his work to a conclusion, and thus it has remained without further significance. The prize problem given by the Paris Academy in 1804, and repeated in following years, led in 1812 to the memoirs of Burkhart and Binet which, however, inspired no further contributions to the solution of the problem of the perturbations of the small planets. Meanwhile an interesting comet was discovered in 1818 by Pons of Marseilles, whose orbit was computed by Encke, and which was on that account called Encke's comet. The aphelion of the comet lies within the orbit of Jupiter, the eccentricity is far greater than that of any of the hitherto known planetary orbits, and the inclination amounts to 12°. If the formulæ could be found which represent the motion of this comet, the question in reference to the small planets would also be solved. It was, however, not merely from this point of view that Hansen set himself the problem of obtaining such formulæ. He doubted, in fact, the correctness of the comet's acceleration found by Encke, and hoped by means of general formulæ to be able to settle

that question. On the basis of the differential equations given in the *Fundamenta Theoria Orbitis quam perlustrat Luna,* Hansen developed formulæ for the perturbations of the logarithm of the radius vector, of the time, and of the sine of the latitude. The essential difference from Laplace's theory consists in the fact that Hansen employs arguments containing multiples of the eccentric anomaly of the comet, of the mean anomaly of the disturbing body. To a given finite power of the ratio of the two semi-major axes there thus belongs a double series, which, with reference to the disturbing body, is an infinite series of powers of the eccentricity, but with reference to the disturbed body is a finite series. This theory Hansen published shortly after 1830 under the title of *Störungen in Ellipsen von grosser Excentricität,* and at the same time made an attempt to obtain the general perturbations of Encke's comet produced by Saturn. Although the computations were not brought to a definite close, still it cannot be doubted that his method is useful for this case. As the perturbations by Jupiter are far more important, both with reference to Encke's comet and to the small planets, and cannot be obtained by this method, Hansen's work cannot be considered as satisfactory, but rather as a failure, at least with reference to Encke's comet. He, therefore, attacked the problem from an entirely different standpoint, and devised the so-called partition method, which he published in his Paris prize memoir, together with an application to the perturbations produced on Encke's comet by the planet Jupiter. This example was also not carried to an end, evidently for the simple reason that this was practically impossible, and thus we see that this method also was unable to solve the problem.

After his unsuccessful effort to obtain general perturbations for such eccentric orbits as that of Encke's comet,

Hansen turned his attention especially to the small planets, and by a further development of the method given in his first memoir succeeded in giving formulæ by means of which he was enabled to represent the motion of the planet Egeria, at least for the time embraced by the observations at his disposal. Unfortunately in this, as in the case of so many other small planets, theory and observation deviated more and more the more distant the latter lay from the epoch of the former, so that after about fifty years, his tables no longer satisfactorily represent the observations. Later several of Hansen's pupils, Lesser, Blecker, and others, computed the general perturbations of some of the small planets. The most prominent of Hansen's pupils, Gyldén, again took up Hansen's partition method and substituted the mean anomaly of the planet and the partial anomaly of the comet by means of elliptic integrals, and thus obtained a much greater convergence in the development of the perturbative function. In the determination of the constants of integration, the old difficulties reappeared, so that taken as a whole no success appears to have been gained. Gyldén sought further, by means of a skillful combination of Hansen's partition method with a special development of series, to obtain a simpler method for the computation of the perturbations of the small planets. Meanwhile all the calculations made in this department of celestial mechanics soon showed that the path laid out by Hansen does not lead to the object desired. Above all, without an immense expenditure of time and labor no trustworthy results can be obtained for planets that occur in the neighborhood of the so-called gaps, for which the terms of long period cannot be accurately determined; and besides, in this case, the convergences of the secular terms are much slower. All attempts in this direction lead only to the result that at best we may obtain in this way ap-

COPERNICUS.

Photogravure from a Painting by O. Brausewetter.

Nicholas Copernicus, the founder of modern Astronomy, was born in Poland, in 1473. He studied philosophy and mathematics at the University of Cracow, and subsequently took a course of law at the University of Bologna. He devoted himself principally, however, to the study of Astronomy, and lectured on Astronomy at Rome in 1500. He studied medicine at Padua about 1501. Meanwhile, he was appointed Canon of the Chapter of Frauenburg, where he spent the rest of his life in the performance of his duties as Canon, and in the practice of medicine.

His famous work "De Revolutionibus Orbium," was written between 1507 and 1530, but it was not till twelve years later that he was induced to have it published, dedicating the work to Pope Paul III. It is related that the first copy of the work arrived a few hours before his death, and that when it was placed in his hands he aroused from his lethargic state for a moment and enjoyed the triumph of his life's work.

In "De Revolutionibus Orbium" Copernicus undertook to demonstrate that the sun is at rest in the centre and that the earth and planets move around it in ellipses, and which scientists now know on unquestionable evidence to be the true doctrine of the solar system.

proximate perturbation formulæ which for a considerable time will guarantee the rediscovery of the planet, without claiming to represent the observations.

The circumstance that a large portion of the small planets occur in the neighborhood of the so-called gaps, thus causing such an increase in the perturbations that after a relatively short time these can no longer be considered as small quantities, led Gyldén to state the question in the following manner: "Will it be possible to determine the elements as absolute constants, and to so determine the terms of long periods (thus avoiding completely the introduction of the time explicitly) that the intermediate orbit thus obtained shall remain included within definite limits, and only differ from the real orbit by quantities of the order of the masses of the planets?" This question includes the question of stability, and the principal problem thus consists in proving the convergence of the long-period series. Gyldén believed that he could establish the convergence by means of what he called the horistic method. Poincaré, however, disputes the correctness of this method. On this assumption Gyldén's theory would be merely an hypothesis. Even if the method is correct, it is applicable only with reference to a limited number of small planets, as it is based upon the development in powers of the eccentricities and inclinations of the disturbing and disturbed planets. Here we stand, so far as this question is concerned, at the end of the nineteenth century. Upon the problem presented at the beginning of the century much skill and labor has been spent; a satisfactory solution has not, however, been reached.

If now we turn to the larger planets, a more gratifying picture presents itself. Here we find at the end of a century a work well completed; taking it all in all, theory has in this case mastered quite satisfactorily the century's immensely rich and abundant observations.

Not only the number of observations made during the first half of the century, but still more their epoch-making precision, which is linked with the names of Bessel and Struve, soon showed that the numerical formulæ of Laplace were not sufficient to satisfy the increasingly accurate observations. In individual cases, it is true, the existing tables of motion were replaced by more accurate ones, as, for example, Hansen's and Olufson's tables of the sun, etc., and Hansen began a new theory of the motions of Venus and Saturn, which he published in his Berlin prize memoirs. Nothing, however, forming a congruous whole was at this time accomplished. The uncertainty of the astronomical constants, the inconsistency between the different determinations of them, the need of a more accurate knowledge of the masses of the large planets for the investigation of the motions of the small planets and comets, the especially unsatisfactory theory of the planet Uranus, all these urged investigators to a thorough revision of the theory of the large planets. At this time, at the opportune moment, appeared the astronomical giant, Leverrier. He was already known to the astronomical world by his work on the secular variations of the orbits of the inner planets, published in the year 1839, when by his wonderful investigations on the motion of the planet Uranus he not only established the existence of an outer planet, but also gave its position so accurately that it was only necessary to direct the telescope to this point of the heavens in order to find it. In connection with the discovery of the planet Neptune, which furnishes one of the most brilliant chapters of the century in celestial mechanics, justice demands that we also mention with equal praise the name of Adams.

Shortly after the discovery of Neptune, Leverrier began the colossal work of the revision of the planetary system, which he was enabled to bring to a conclusion. Lever-

rier planned his work clearly and systematically, and clearly and systematically carried it out. Lagrange's method of the variation of constants proved its power in the most splendid manner. Mathematician and astronomer, Leverrier gave in a peculiarly harmonious combination only the necessary formulæ and these in the simplest manner, and so arranged the astronomical material as most completely to suit his problem. I am convinced that, no matter how many new revisions may be necessary, Leverrier's *Annales de l'Observatoire de Paris* will never be forgotten.

The theory of the inner planets was completed at the end of the sixties, whence new and much-needed values of the masses of the planets Mercury and Venus, as well as of the solar parallax, were obtained. As a result of this investigation it was discovered that the motion of the line of apsides of the planet Mercury was not represented satisfactorily by the theory, whence Leverrier assumed the existence of an unknown intra-Mercurial planet as the cause of the perturbations.

What disappointment Leverrier met at that time is well known, but it is also well known that nothing could turn him from his devotion to the great problem, nothing could bend the force of his great genius. Even after his flight from Paris a lead pencil and a copy of logarithm tables furnished a sufficient means for carrying on the theory of Jupiter. The work was completed not long before his death, and science possessed a theory of the motion of that great planet carried out in a remarkably homogeneous manner; even the theory of Saturn, after a few additional computations by Gaillot, could be considered satisfactory.

In spite of the advances which Leverrier's work shows, astronomy needed another giant to reach the standpoint which it has gained during the last century; the name of this giant is Newcomb. A colossal conscious force, the

most comprehensive theoretic knowledge, an acquaintance with observing material and its significance extending to the smallest details, were necessary conditions for the undertaking, immediately after Leverrier, of a revision of the planetary theory. During the last half of the century a mass of observations, rich in quantity and quality, had been gathered, which Leverrier had been unable to use; moreover, additional determinations of certain astronomical constants gave values, which, in consideration of the great accuracy now demanded, it was necessary to take account of in place of those employed by Leverrier. Newcomb's great aim was to obtain a system of astronomical constants and elements of motion which should be as unified as possible, and should correspond with the progress made in the art of observing. The theories of the planets Neptune and Uranus which Newcomb published about 1870, but above all his *Catalogue of Fundamental Stars,* seemed to be precursors of the *Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac.* The catalogue mentioned is especially important, since it forms, in a certain sense, an epoch in the systematic treatment of observations, and the preparation of them for the service of theory. What Newcomb did for the right ascensions, Boss has done for the declinations. In the *Astronomical Papers* we find, then, *summa scientia astronomica,* in all questions that refer to the solution of the problem under discussion. The developments of the theories of the planets, while in agreement with the general fundamental principles of celestial mechanics, are especially adapted to the individual cases, varying in method as the problems demand, and are always so explained as to keep clearly in view the object to be attained. In this work, beside the name of Newcomb shines that of the great mathematician, Hill, who has made available for the advancement of astro-

nomical research the almost forgotten treasures of the immortal Gauss, and was the first to apply successfully Hansen's method to the computation of the mutual perturbations of Jupiter and Saturn. The astronomical papers are thus valuable, not only on account of the results themselves, but also by reason of the methods by means of which these were attained; that is, in other words, these papers have brought celestial mechanics, both in form and content, to a definitely higher plane. Newcomb's little book, *Astronomical Constants,* gives in concise and clear form a bird's-eye view of the results of this work. Newcomb has succeeded in attaining essentially what he started to attain; he has contributed to science a homogeneous system of the fundamental constants of astronomy; to his energy, almost bordering on the wonderful, we are indebted for the realization of the most valuable results at present attainable from modern observations. This view was expressed by the Paris Congress of 1896, when it accepted Newcomb's system almost unchanged; and if it were to assemble again to-day it would certainly correct a small error which it committed. The determination of the precession constant by means of the stars, which remained to be accomplished, as well as the formation of the fundamental catalogue of stars, was delegated to him and was practically accepted in advance, an evidence of the unlimited trust in the authority of Newcomb.

Among the improved values of the masses which result from Newcomb's theory, I should like to call especial attention to the mass of the planet Mercury, which is 30 to 40 per cent smaller than that obtained by Leverrier. On account of the smallness of the coefficient of the mass in the equations of condition, this is very difficult to obtain. Now, however, it has been obtained in another manner and independently, whence it arises that it may be con-

sidered as correct within its probable error; this proves again the rigor with which the calculations in the *Astronomical Papers* have been carried out. The motion of the line of apsides of the planet Mercury, not explained by theory, which was discovered by Leverrier, is confirmed by Newcomb. The explanation of this motion will have to wait for further astronomical discovery.

One of the most beautiful discoveries of the century was that of the satellites of Mars. The new problem in celestial mechanics arising therefrom was solved by the discoverer. The fifth satellite of Jupiter, which was added to science by the distinguished observer, Professor Barnard, has added another very important theoretical problem. If we glance now over what has been presented, it cannot be denied that celestial mechanics, during the past century, especially with reference to the motions of the major planets, has essentially kept pace with the results of observation, and that as a whole it satisfies the enormously improved methods of observing. The last thirty years of the past century belong, in this respect, to America, and I believe that every European astronomer will agree with me that they are also the most important.

The progress made in the field of lunar theory has not been mentioned. To do this, however, I should have to explain the works of Poisson, Plana, Hansen, Delaunay, Newcomb, Adams, Tisserand, Hill, and many others, which would require an address at least as extended as the present. I limit myself, therefore, to reminding you that the lunar theory offers yet unsolved problems to the theoretical astronomer, in spite of the splendid results of the savants named, and in spite of the fact that Newcomb has succeeded in improving Hansen's Lunar Tables, and that at present these represent the observations well.

I have purposely omitted the names of Jacobi and Ham-

ilton, so well known in celestial mechanics, whose theories have received further development and application from Delaunay, Tisserand, and Hill, and have served Poincaré as a starting-point for his remarkable theories, beginning with his beautiful Prize Memoir and continuing through the *Méthodes Nouvelles de la Mécanique Céleste,* etc. I do not feel justified in expressing myself with reference to the value and meaning of this last work, for the simple reason that I am not mathematically competent to do so. That Hill's and Poincaré's theories introduce a new epoch, whose fruits the twentieth century will harvest, there seems to exist no doubt.

The nineteenth century has added a new chapter in celestial mechanics, the theory of meteors and comets in their relation to one another. We owe to the clever researches of Schiaparelli, Newcomb, Bredichin, and others the remarkable insight obtained into the motion of these small bodies, which remain individually invisible, except when they penetrate our atmosphere and blaze up, or become visible by being crowded together in the form of comets.

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THE RELATIVE VALUE OF GEOGRAPHICAL POSITION

BY HENRY YULE OLDHAM

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THERE is a factor involved in the consideration of many geographical problems which is too commonly overlooked. That factor is the element of time. Familiarity tempers human judgment, and the constant obtrusion of the more obvious naturally induces oblivion with respect to the remoter aspects of a case.

The New World looms so large in modern life that it is difficult to remember that till comparatively recent times in the history of mankind it was practically non-existent.

Ever since there has been an atmosphere surrounding the globe, there have probably been steady easterly winds in the tropical regions, with stronger but less regular westerly ones in the temperate climes. The sea, in essence a vast body of cold water, with a shallow upper layer of warm, in obedience to the working of the winds shows a tendency in the tropics to a heaping-up of the warm surface water on the eastern sides of continents towards which the winds blow, with an upwelling of the colder lower layers on the western sides, where the wind blows off the shore; while in the temperate zones, here the wind are reversed, the positions of warm and cold water are naturally also reversed.

The temperature of the sea has a marked effect on the life of the organisms which dwell in it, while its influence on the atmosphere above produces notable climatic effects on the land. Thus the warmer water on the eastern shores in the tropics conduces to the growth of coral reefs, which are as markedly present on the eastern coasts of Australia, Africa, and America, as they are conspicuous by their absence from the western. Similarly the contrast in the temperate zone, between the warm, moist climate of British Columbia and the frozen wastes of Labrador, is no less striking than the difference between the climates of western Europe and eastern Siberia.

Were there any new continents to be discovered, one could predict that their western shores would be warm and wet in temperate regions, their eastern ones in the tropics.

These are some of the salient and constant factors in geography. Such also are the distribution of land and water, the configuration of the continents, their streams and mountains, their valleys and plains, which have changed but little in historical times; but familiarity with these features tends to forgetfulness of the fact that, like actors on a stage, their appearance in the theatre of history has been gradual, and that, though their actual positions have remained unchanged, their relative positions have varied through the ages, and moreover that they are often destined to play more parts than one.

A sea like the Mediterranean may at one time be the centre of commercial activity, and then become a backwater, while commerce streams along an ocean route round Africa. A few centuries pass, and the cutting of the Suez Canal, coupled with the development of steam navigation, restores it to its ancient and honorable estate as a highway of communication with the East, and the great cities on its shores, like Venice and Genoa, after a long period of decay, begin to resume their pristine vigor.

Since long before the beginning of human life, stores of gold and coal and other minerals have lain in the bosom of the earth; but their development as the sites of great centres of population has in most cases been essentially linked with the element of time. The rapid growth of Johannesburg into the position of the largest city of South Africa would have been as impossible without the recent discovery of the cyanide process, as was the development of the great coal-fields—the most striking factor in the shifting of great masses of population in modern times—until the invention of the application of steam power to machinery.

The great forces of nature show little tendency to change and may be usefully applied to the elucidation of many geographical problems, as for example in the case of the early voyages round the Cape of Good Hope. Bartholomew Dias, the first discoverer of the cape, encountering adverse westerly winds on his return voyage, must as assuredly have been there in our northern summer months, when the shifting of the trade-wind system brings the cape in the influence of the westerly winds, as was Vasco da Gama there in our winter months, when easterly gales prevail. Similarly the voyage of Odysseus may be largely elucidated with the help of a modern manual of sailing directions for the Mediterranean. But it is into the fluctuating fortunes of individual districts that the time-factor chiefly enters and demands a nice discrimination between the meaning of actual and relative geographical position.

Potentiality precedes performance. An island may be long the home of a hardy race of mariners before a field adequate to the display of their abilities is unveiled. The site of a great city may seem to have been predestined for centuries, before the opportunity for its development arises. Just as the obstacle presented by the lack of light in the

short winter days of northern latitudes has been overcome by the invention of modern artificial illuminants, so the present barrier of unhealthiness to the development of the tropics may be removed by the discoveries of medical science. A place of business on the outskirts of a city is at a great disadvantage compared with one situated in the centre, but the expansion of the town may in course of time completely reverse their relative positions, without the smallest variation in their actual sites being made.

An interesting example on a small scale is presented by the fortunes of a famous English school. In the reign of Queen Elizabeth, a Warwickshire lad, Laurence Sheriffe by name, left his boyhood's home at Rugby to win fame and fortune in London. Mindful of his early days, and wishful to help succeeding generations, he left by will two fields in the neighborhood of Rugby, to provide the means for obtaining the aid of, if possible, a Master of Arts to teach the boys of his native town. By a fortunate inspiration, in a codicil to his will, two fields in the neighborhood of London were substituted for the original pair in the neighborhood of Rugby. At the time the two portions of land were probably of nearly equal value, but, though their actual position has never changed, their relative positions have undergone a revolution. The two fields near Rugby remain two country fields of little worth; the two near London in the reign of Queen Elizabeth are now in the heart of the great metropolis, and produce a princely revenue, on which the fortunes of the school at Rugby have been raised.

An example on a larger scale is offered by the history of England. It is a truism, often repeated, that the British Islands lie almost in the centre of the land-masses of the globe, an unrivaled position for wide-reaching empire and dominion. But this relative position is only one of recent

growth. Five hundred years ago, a short period in the history of man, England was in a position of isolation on the very outskirts of the then known world. Shut in by the pathless barrier of the Atlantic on the west, and the untrodden wastes of Africa in the south, the only outlook of Europe was towards the east. Then was the Mediterranean, as its name implies, literally in the centre of the earth, the great scene of maritime activity. The principal nautical charts of the early fifteenth century, the Italian *portolani,* admirably reflect this state of affairs. The major part of the map is occupied by the Mediterranean, whose shores are studded with ports; a few of these on the west of Africa as far as the latitude of the Canaries, and several on the west of Europe as far as Flanders, indicate the limits of ordinary navigation. England, with only a few ports, chiefly on the south coast, is in the extreme corner of the map, separated by a long and hazardous sea-voyage from the great centre of activity. Under such conditions the central situation of Italy gave it a predominant position, and Venice and Genoa became the natural foci of commercial power.

It was only natural, also, that Italy should prove the birthplace of the great pioneers of geographical exploration. From Venice came Marco Polo, the great explorer of Cathay, who first to Western eyes unveiled the wonders of the East, and through whom Venice learned "to hold the treasures of the gorgeous East in fee;" from Genoa, Columbus, the pioneer of Western exploration, who sought, but failed, to find a western route to the Indies, and in his failure won a greater fame by the revelation of the road to a new and unsuspected world; while Florence saw the birth of Amerigo Vespucci, the scholarly explorer, who first realized that this new world was totally distinct from Asia, and so led to his name being inseparably linked with

it. Cadamosto of Venice, sometimes called the Marco Polo of West Africa, the Cabots and Verrazano, pioneers of Western exploration for England and France, were likewise Italians.

The trade with the East, the home of silks and spices,—some once almost worth their weight in gold,—was till recent times the prize of the world's commerce. It was the fertilizing streams of Eastern commerce, pouring into the Mediterranean by various routes, but mainly up the Red Sea, which nourished Genoa and Venice. But gradually round the Levantine shores there spread, eventually from Cairo to Constantinople, the Turks, an alien race of alien religion; and Turkish dues, exacted on the inevitable land transit across Egypt from the Red Sea, proved a serious and increasing charge on the profits of this commerce.

In the latter part of the fifteenth century a merchant of Venice, writing to the King of Portugal, said that the greatest trade of Venice was with India, which came by way of Alexandria, whence the Turk derived great profit; he could not say where India was, but it was an affair for a great prince to undertake to find it, for if successful he would be exalted in riches and grandeur above all others.

The necessity of finding an ocean highway to the East, which would obviate the need of any land-break, with all its consequent expenses, had, however, been anticipated at an earlier period. The natural direction in which to seek such a route was round Africa. No one knew whether this were possible, or even if Africa had a southern end, but it was probable, and, indeed, the impartial record of an incredulous historian, Herodotus, had handed down the tradition of a Phenician circumnavigation of the continent six hundred years before the beginning of the Christian Era.

For the quest of a route round Africa, the relative posi-

tion of the Iberian Peninsula at the time foreshadowed the preëminence of Spain or Portugal. That the initiative came from Portugal was partly due to the fact that that country had freed itself from the Moors, before the Christian reconquest of Spain was complete, but chiefly to the birth of one of those remarkable personalities that leave a permanent mark on the history of mankind.

In A. D. 1415, when just of age, Prince Henry of Portugal, third surviving son of the reigning king, distinguished himself so preëminently at the capture of Ceuta that he was offered the dignity of knighthood before his elder brothers, an added honor which he modestly declined. The fame of his attainments brought brilliant offers from other countries, but all were refused. Accepting the governorship of the southern province of his native land, he settled at Sagres near Cape St. Vincent, and practically devoted the rest of his life to one great idea,—the unveiling of the coast of Africa, in pursuance of the search for an ocean highway to the East. It is not easy to realize the difficulties that checked the work: the terrors of the unknown, the superstitions of his sailors, which long prevented their penetrating beyond the latitude of the Canaries, then the farthest limit known along the western coast of Africa. His indomitable persistence, however, prevailed. Gradually the inhospitable edge of the Sahara was passed, and the rich region of Senegambia discovered, so that, ere his death in 1460, Cape Verde, the westernmost point of the continent, had been rounded, and a district a little beyond the Gambia reached, while the island groups of the Madeiras, the Azores, and the Cape Verdes had been added to his country's dominions.

Compared with the long stretch of the African coasts, this may seem but a small achievement, but in itself is an indication of the initial difficulties to be overcome.

The first step had been taken; the rest was comparatively easy. As an example of the far-reaching designs of Prince Henry, it might be noted that at an early stage he obtained from Rome Papal bulls granting to Portugal all countries found, not in the possession of a Christian monarch, *usque ad Indos*. And so, after his death, the quest for the Indies was resumed, and gradually the long eastern trend of the Guinea Coast explored, and then the still longer southern stretch, until, after a little more than twenty-five years had elapsed, an end to Africa was found, and a cape hard-by happily named the Cape of Good Hope. The way seemed clear, and ten years later was proved to be so, when the gallant Vasco da Gama led the first expedition along a continuous ocean highway from Europe to India.

The first shot fired by the Portuguese on the Malabar Coast of India was the signal for the downfall of the commercial supremacy of Venice, and for three and a half centuries the great trade with the East was diverted, for some time to the exclusive benefit of Portugal, from its normal and ancient route up the Red Sea into a new Atlantic path, until the old order was restored by the cutting of the Suez Canal.

The century-long quest of the Portuguese to find this way round Africa was not likely to pass without some rival routes being advocated, and one there was which had a classic flavor.

To reach the East by sailing west was a natural corollary to the demonstration that the world was globular. Many of the Greek geographers had spoken of it, and though the famous Eratosthenes—who in the third century B. C. had measured the size of the earth with greater accuracy than any one attained to until quite modern times—had dismissed the scheme as impracticable owing to the extent of intervening ocean, the later Ptolemy, with

restricted ideas as to the size of the earth and exaggerated notions of the extent of Asia, made it appear but a short voyage from the west of Europe westward to the east of Asia. This scheme, first mooted about the middle of the fifteenth century by Paul Toscanelli, an astronomer of Florence, won little sympathy from the Portuguese, who were rightly committed to the African route, but found an ardent advocate in Columbus.

After long waiting, in 1492, the consolidation of Spain, accomplished by the eviction of the Moors from their last stronghold in Granada, gave Columbus his opportunity, and in the service of Spain, the second of the two countries occupying the favorably situated Iberian Peninsula, he set out on his famous voyage, as the pioneer of Western exploration. A short voyage of less than five weeks from the Canaries, helped by the favoring trade-winds, revealed land, where land was anticipated. Asia had apparently been reached at the first attempt, by the easiest of voyages, and the name West Indies perpetuates the blunder to this day.

Other voyages quickly followed, and presently the great wonder of a new and unsuspected world was revealed, lying like a great barrier to the immediate object of the Western quest, but instinct with the greatest possibilities. A new route to an old world had not been found, but the path to a vast new continent, hitherto undreamed of, had been laid bare.

Twenty years after Columbus's first voyage, the sea that lay beyond the New World was first beheld by Nunez de Balboa,

> "When with eager eyes
> He star'd at the Pacific, and all his men
> Look'd at each other with a wild surmise—
> Silent, upon a peak in Darien."

But for all Keats's fine imagination, the marvel of the Pacific was as unsuspected as had been the existence of the

New World. America was supposed to lie close up to Asia and only separated from it by a narrow sea.

It was nearly ten years later that Magellan, a native of Portugal in the service of Spain, found a passage through the straits which bear his name, at the southern end of the barrier continent, and, after a voyage of unrivaled difficulties, revealed the vast extent of the Pacific Ocean, which covers nearly half the whole surface of the globe. In this notable voyage,—the most notable, as a contemporary chronicler quaintly remarks, since that of the patriarch Noah,—the East Indies, where the leader lost his life, were reached by a western route, while one ship out of five completed the circumnavigation of the globe with a handful of men, who on their return crawled as humble penitents in sackcloth and ashes through the streets of Seville, because, having unconsciously lost a day in the voyage, they found that they had been keeping the fasts and festivals of their Church on the wrong dates.

With the unveiling of the Atlantic in the fifteenth century,—the conversion of what had been a pathless barrier into a great field for maritime activity,—a new era begins, the medieval Mediterranean epoch closes, and the modern oceanic period succeeds. The relative value of the position of the Iberian Peninsula for carrying out this great work was so preëminent that for some time Portugal and Spain were suffered to proceed unrivaled and unchecked. Indeed, by mutual agreement, a line of demarkation was drawn from north to south, about through the mouth of the Amazon, by which the whole undiscovered portions of the world were divided into two hemispheres, an eastern one for Portugal, a western one for Spain. But the very success which had been won wrought a revolution in the relative positions of the other lands in western Europe. England and France were equally well placed for undertaking western voyages.

It was the King of France who, in the sixteenth century, is said to have ironically invited Portugal and Spain to produce the will of our father Adam which constituted them his sole heirs. It was England, however, which mainly profited by the great change. Our island race of bold and skillful navigators had been only waiting for the opportunity of a field adequate to the display of latent powers. The time had come, and with the reign of Queen Elizabeth in the second half of the sixteenth century begins the expansion of England.

Sir John Hawkins was one of the first to dispute the exclusive right of Spain to traffic with the West Indies. Sir Francis Drake, the first to rival Magellan as a circumnavigator of the globe, was the most brilliant leader in the long struggle for the mastery of the sea which led up to the great tragedy of the Spanish Armada. Sir Walter Raleigh, no less an organizer of exploration than an explorer himself, by his attempts to colonize Virginia laid the foundation for the Anglo-Saxon dominion of North America.

Raleigh, Drake, and Hawkins, with most of their associates, were all Devon men, and this was only to be expected, for the position of Devon at the southwest corner of the land bears the same relation to the rest of England as in the earlier work the Iberian Peninsula bore to the rest of Europe, giving the Devon men for the time a positive advantage in the voyages undertaken to the famous cry of Westward, Ho! The period of their activity is ever recalled by the happy rhyme, which couples the dashing Drake with the famous Virgin Queen—

> "Oh! Nature, to old England still
> Continue these mistakes;
> Still give us for our Kings such Queens,
> And for our Dux such Drakes."

It was an English merchant, resident in Spain, who first suggested that, if feasible, a polar passage to Cathay would prove the shortest route, shorter than either the Portuguese path round Africa or the Spanish one across the Pacific, and that England was most favorably placed for undertaking the attempt to find one.

Attempts were accordingly made to discover a northeast passage, but soon a rival was found in the Dutch, who were equally well placed for such an undertaking. That the passage should eventually be completed long afterwards by Sweden is appropriate, when the position of that country is remembered.

It is, however, rather with the long search for a northwest passage that our countrymen are associated. From the time of Sir Martin Frobisher, the Columbus of the scheme, Davis of the Straits, and Baffin of the Bay, their names have been written largely on the map of North America, until the last link was forged with the life of Sir John Franklin.

When once England had ceased to lie on the outskirts of the known world, and had by the course of events become the centre of the land-masses of the globe, the path was clear to supremacy in maritime affairs. That the brilliant achievements of the sixteenth century were not continued in the seventeenth was due to internal political conditions. A century that saw the unhappy introduction of the Stuart dynasty, and its collapse after all the horrors of civil war, was not favorable to external development. A period of internal commotion is not adapted to external activity. Consequently, it is rather with the Dutch that the honors of exploration in the seventeenth century must rest. Boldly disputing the monopoly of the Cape route to the East Indies, they obtained a footing among those islands, and from that vantage-point prosecuted the unveiling of the great adjacent continent of Australia.

Unfortunately the region of New Holland, as they called it, which was first discovered, was mainly the arid western parts, and even when the continent was circumnavigated by Tasman, the fertile eastern coast was entirely missed. Hence, for the Dutch, Australia remained a region of possible future colonization, rather than one to be readily exploited.

A whole century was destined to pass before the fertile eastern shore was to be revealed, and then by a sailor of another nation, the English Captain Cook, who, after sailing in and out round the islands of New Zealand, of which Tasman had only seen a fragment, explored the whole of the east of Australia, and so opened the road to its colonization by a different nation from the Dutch.

The name of Captain Cook serves as a reminder that the eighteenth century saw a revival of maritime activity in England. It is with the great Pacific Ocean that his name is inseparably connected; east and west, north and south he penetrated to its utmost limits, revealing much of its wealth of islands, and finally sinking to rest in its waters, slain, like his great predecessor Magellan, in a petty skirmish, while endeavoring to protect his men.

Cook was the last of the great oceanic explorers. After him sailors were left, like Alexander, sighing for new worlds to conquer.

The nineteenth century, save for attempts to penetrate the polar fastnesses, has been mainly concerned with the exploration of the interior of continents, in which representatives of many nations have been engaged, for none have had special advantages of position.

The development of steam navigation has largely served to annihilate distance, and has destroyed much of the relative value of position, which gave some countries an advantage in earlier times, under other conditions.

One interesting result has been a revival of the early Italian eminence in exploration, the Duke of the Abruzzi's expedition having penetrated to the "Farthest North" yet reached, while in the recent attack on the Antarctic there has been a striking combination among a large number of countries.

Finally, the fact that the great Universal Exposition is held this year at St. Louis, where we are assembled, in the heart of North America, suggests a reflection on a change in relative position, which has affected many districts at different epochs, owing to a tendency for the spread of civilization to follow the course of the sun in its westerly path,—as Wordsworth puts it, "Stepping westward seem to be a kind of heavenly destiny." To the Assyrians of old, Europe itself was the West—*Ereb;* Moorish names in Portugal and Morocco represent the West of a later period, while Cape Finisterre similarly records the limit of the land. In the New World, the same phenomenon repeats itself, the centre of gravity in the distribution of its population moves steadily to the west, and the name of the "Far West" is losing its earlier significance. Already for some time the waves of civilization have reached the far Pacific shore.

One thought remains. The Middle Ages might fittingly be described as a Mediterranean epoch. Then followed the Atlantic period of modern times. The problems of the future seem largely bound up with the Pacific, and, indeed, signs are not wanting that we are entering on a new era.

THE RELATION OF OCEANOGRAPHY TO THE OTHER SCIENCES

BY SIR JOHN MURRAY

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WITHIN the past half-century our knowledge of the ocean has been very greatly extended by the explorations of scientific men belonging to nearly every civilized country. The depth of the ocean, the temperature, the composition, and the circulation of ocean waters, the nature and distribution of oceanic organisms and of marine deposits over the floor of the ocean, are now all known in their broad general outlines. We are at last in a position to indicate, and to speculate concerning, the relations of oceanography to the other and older sciences.

We now know that the greatest depth of the ocean below sea-level exceeds the height of the highest mountain above the sea-level. If Mount Everest, the highest mountain peak in the world, were placed in the Nero Deep in

the North Pacific, where a depth of 31,600 feet has been recorded, its summit would be submerged by about 2600 feet, and if placed in the Nares Deep of the North Atlantic, where 28,000 feet have been recorded, it would form a small islet 1000 feet above the waves.[1] We now know about eighty-six areas in the ocean where there are depths exceeding three geographical miles (3000 fathoms). These areas, in which depths greater than 3000 fathoms have been recorded, have been called *deeps,* and a distinctive name, like *Nero Deep* and *Nares Deep,* has been given to each one of them. On the other hand, there are in the ocean basins numerous cones, rising in some instances above sea-level and forming coral and volcanic islands, or rising it may be to a few hundreds of feet below the sea-level. These elevated cones rising from the ocean's floor seem for the most part to be of volcanic origin; when they do not rise to the sea-level they are covered with a white mantle of carbonate of lime shells, mostly of plankton organisms where their summits are submerged half a mile or more. Disregarding these elevations and depressions, which are after all of small extent, it may be said that the general level of the bed of the great ocean basins is submerged about two and a half geographical miles beneath the general level of the surface of the continents. Were the ocean waters run off the globe, the solid surface of the sphere would appear like two great irregular plains, one of which —the continental areas—would be elevated nearly three miles above the other,—the floor of the great ocean basins; this is the fundamental geographical fact. In comparison with the size of our globe, this may seem a very small matter; still, it is important to inquire whether or not this great superficial appearance of the solid crust is part of its original structure, or has been brought about by agencies

[1] Greatest depth in the Pacific (Nero Deep) = 5269 fathoms; greatest depth in the Atlantic (Nares Deep) = 4662 fathoms.

at work since the first crust was formed over the globe's incandescent surface, or since the first precipitation of water on the surface of our planet.

Geodesists tell us that their observations point to a deficiency of matter beneath the continental areas, and it seems possible that oceanographical researches may give some hint as to how this deficiency of matter may be accounted for. It is probable that most of the chlorine and sulphur now in combination in the ocean were carried down from the atmosphere with the first calls of rain on the surface of the primitive crust, in which we may suppose that all the silica was combined with bases, such as the alkalies, lime, magnesia, iron, manganese, and alumina. At a high temperature silicic acid (SiO_2) has a great affinity for bases, but at a low temperature it is replaced by carbonic acid (CO_2), which resembles silicic acid in many of its properties; geological History might indeed be represented as a continuous struggle between these two radicals for the possession of bases. At a high temperature SiO_2 is successful, while at a low temperature the victory rests with CO_2. In all the ordinary disintegrating processes at work on the surface of the earth since the first precipitation of rain, carbonic acid has been replacing silica from its bases; a large part of the silica thus set free goes to form the hydrated variety of silica, like opal, or ultimately free quartz. The bases are thus continually being leached out of the emerged rocks of the continents, and carried away to the ocean in solution, or in a colloid condition, the result being the ultimate deposition of the greater part of the heavier materials in the abysmal regions of the ocean and an accumulation of the lighter refractory quartz on or near the continental areas.

A detailed study of marine deposits shows that, while quartz-sand forms generally the largest part of the de-

posits close to the continents, quartz-sand is, on the other hand, almost wholly absent from the abysmal regions of the ocean far from land, except where the sea surface is affected by icebergs. The average chemical composition of terrigenous deposits near land and of continental rocks shows about 68 per cent of free and combined silica. On the other hand, the average chemical composition of abysmal deposits shows only 36 per cent of silica. Continental rocks have an average specific gravity of 2.5. The abysmal deposits now forming on the floor of the ocean would make up rocks with a specific gravity of over 3.1. The superficial layers of the earth's crust on the continents must be, therefore, considered specifically lighter than the superficial layers of the earth's crust below the waters of the ocean.

Everywhere along continental slopes and in inclosed seas we find a series of strata being formed in all respects comparable with the stratified rocks of the geological series. Glauconite is to be found now forming on nearly all continental slopes, but it is not met with in the deep-sea deposits far from land. This same mineral is found in the same form as in recent deposits throughout the whole series of stratified rocks from the pre-Cambrian down to the present time, so that it is legitimate to assume that rocks in which it occurs were laid down close to continental land. Phosphatic nodules are very intimately associated with glauconitic deposits, and have the same distribution in the present seas; they are found along the submerged slopes of continental land, and are very rarely met with in deep water far from continental shores. There is a similar association of phosphatic nodules and glauconite throughout the whole geological series of past ages. These phosphatic and glauconitic rocks are now forming especially where ocean currents from different sources and of different tem-

peratures meet, as, for instance, off the United States coasts in the Atlantic and Pacific, off the Cape of Good Hope, off eastern Australia, off Patagonia, and off Japan. In these areas there is a vast destruction of life, owing to sudden changes of temperature of the sea-water. The organisms in the cold current are killed from a sudden rise of temperature, the animals in the warm current from a sudden lowering of the temperature. When the tile-fish was nearly exterminated off the United States coasts in 1882, it was estimated that over hundreds of square miles there was a layer of dead marine fishes and other animals on the bottom six feet in depth. This vast destruction of marine organisms points clearly to the source of the phosphate in these deposits, and we obtain a hint as to the conditions under which greensand and similar rocks were laid down in past ages. Generally it may be said that in the terrigenous deposits along continental shores we have rocks now in process of formation which resemble closely the stratified rocks of the continents, so that these rocks may all be said to have been formed in varying depths in inclosed seas or along the continental slopes within two or three hundred miles from land.

It is quite different when we turn to the marine deposits now in process of formation towards the central portions of the great ocean basins. No geologist has yet been able to produce a specimen of a statified rock which can with certainty be said to have been built up under conditions similar to those under which the typical red and chocolate clays, the Pteropod and Globigerina oozes, the Radiolarian and Diatom oozes of the central oceanic regions are laid down at the present time. These pelagic deposits cover considerably more than one half of the surface of our planet. The typical pelagic deposits are principally made up of the shells and skeletons of calcareous and siliceous organisms

now living in the surface waters and of inorganic material derived from submarine eruptions, or of pumice and volcanic dusts floated or wind-borne from volcanic areas. The calcareous organisms play a most important rôle in the pelagic deposits, and their greater or less abundance, or complete absence, is more or less puzzling to the oceanographer. If, for instance, we should find in the tropical or subtropical regions of the ocean a cup-shaped or horseshoe-shaped elevation rising from the deep floor of the ocean, having a diameter, say, of fifty miles across, and the summit or edges of the cup rising to within 6000 feet of the surface of the ocean, while in the interior and on the outside of the cup the bottom descended to 20,000 feet below the waves, then we should find on the elevated edges of the cup deposits made up of 90 per cent of calcium carbonate, consisting almost wholly of the remains of pelagic organisms. As we descend into the hollow of the cup, or into the depths outside the cup, these organic remains would slowly disappear, till in the deposit at the bottom in 20,000 feet hardly a trace of calcareous organisms would be found, and the deposits there would consist of a red or chocolate clay derived from volcanic ejecta, with manganese-iron nodules, earbones of whales, sharks' teeth, and some cosmic spherules derived from meteorites. This hypothetical case represents what is found again and again throughout the ocean basins. Where exactly similar surface conditions prevail at the surface of the ocean two wholly different marine deposits are being formed on the floor of the ocean, the only varying condition being depth. The calcareous organisms are all dissolved away in falling through an ocean 20,000 feet in depth, or soon after they reach the bottom, whereas they nearly all reach the bottom at a depth of 5000 or 6000 feet, and there accumulate so as to form an almost pure deposit of carbonate of lime. The clayey deposit at

20,000 feet evidently accumulates with extreme slowness, the calcareous deposit at 6000 feet much more rapidly. The recent observations of telegraph engineers appear to show that, at one place in the North Atlantic, Globigerina ooze forms at the rate of about one inch in ten years.[1] In the case of terrigenous, as well as of pelagic deposits, it has been shown that two very different deposits, both in organic and inorganic constituents, may be formed in the same area at the same time, but in different depths.

All these considerations go to show that the deposits formed in inclosed seas and along the borders and slopes of emerged continental land have again and again been shoved up on the continental areas to form dry land, by the action of those internal forces called into play through the solid crust acommodating itself to a shrinking nucleus. And, further, it follows that more than one half of the surface of the planet—the abysmal regions of the great ocean basins—may never have contributed to the formation of those stratified rocks of which continental land is so largely made up. The continents have been far from permanent and stable, but those areas on the surface of the planet now occupied by the continents and the adjacent marine terrigenous deposits appear, from the foregoing argument, to have been the areas on the surface of the planet on which continental land has been situated from the very earliest ages. The grand result of all the denuding and reconstructing agencies since the first precipitation of rain has been the building-up on these continental areas of a great mass of lighter highly siliceous materials. If this has been the course of the evolution of the present continental areas, then it appears amply to account for the deficiency of matter beneath the continents indicated by pendulum observations,

[1] See Murray and Peake, *On Recent Contributions to Our Knowledge of the Floor of the North Atlantic Ocean, Roy. Geogr. Soc.*, Extra Publication, 1904, p. 21.

and for the alleged fact that along continental shores the plumb-line tends towards the ocean basins, where the heavier materials have been accumulating on the earth's surface, ever since the first precipitation of water on the cooling crust.

Temperature may be defined as that state of matter on which depends its relative readiness to give or to receive heat. Variations of temperature are intimately associated with all changes in nature, and nowhere are the effects of these variations of temperature more pronounced than in the ocean. The relations of oceanography to many other sciences can best be exemplified by a consideration of the distribution of temperature in the waters of the ocean. Nearly all the sun's rays falling on water are at once diffused downwards to at least 600 feet. So great is the thermal capacity of water that a unit of heat only raises the temperature one degree, while the same amount will raise the temperature of rocks four or five degrees.

It is well known that our planet is surrounded by three atmospheres: one of oxygen, one of nitrogen, and one of water-vapor. In the case of oxygen and nitrogen a complete mixture takes place throughout the whole atmospheric envelope. A complete mixture never takes place in the case of water-vapor, because its equilibrium is continuously disturbed by changes of temperature, which may reduce the vapor to the liquid or solid state; evaporation and condensation, freezing and melting, are ceaseless at the surface of the earth. It has been shown by numerous observations that in the open ocean far from land the daily fluctuations of temperature in the surface waters do not exceed one degree F. Hence the atmosphere over the ocean may be regarded as resting on a surface the temperature of which is practically uniform at all hours of the day. This is in striking contrast to what takes place on the land sur-

faces, where solar and terrestrial radiation produce a very wide daily range of temperature. On the sand of the Sahara and the American deserts the temperature ranges about 100° from three A. M. to three P. M. The temperature of the air immediately over the ocean has a slightly greater daily range than that of the water,—being some three or four degrees F.,—but this is no way comparable to the enormous daily range of the air resting on the land surfaces. Here we come on one of the prime factors of meteorology, which must be considered in connection with some other facts. As the diurnal oscillations of the barometer occur alike over the sea and land, it follows that this diurnal oscillation is caused by the direct and immediate heating of the molecules of the air and its aqueous vapor by solar radiation. Air with a large quantity of water-vapor absorbs more of the sun's rays, becomes in consequence more heated, and is specifically lighter than dry air; hence air ascends in cyclonic and descends in anti-cyclonic areas. The diurnal variation in the elastic force of the vapor in the air is seen in its simplest form over the open ocean, and the diurnal variation in the force of the wind, and the diurnal variation in the amount of cloud are both much less over the open ocean than over the land. All these conclusions derived from observations at sea go a long way towards a rational interpretation of many atmospheric phenomena, such as the unequal distribution of the mass of the earth's atmosphere, the ascending currents in cyclonic areas, the descending currents in anti-cyclonic areas, the prevailing winds, and the greatly diversified climates in different parts of the world. The aqueo-aërial currents from sea to land, and the oceanic currents thus brought about by changes of temperature in the atmosphere, are the great equalizers of temperature in diverse regions; for instance, except for these currents the mean winter tem-

perature of London would be 17° F. in place of 39° F., London thus being benefited 22° F., while the Shetland Islands to the north of Scotland are benefited 36° F. by the Gulf Stream and the aqueo-aërial currents due to the winds from the southwest.

At the surface of the earth, both on land and on sea, bands of equal temperature run more or less parallel to the equator. This is true, notwithstanding the fact that oceanic currents cause wide deflections, as, for instance, in the case of the Gulf Stream: on the sea-floor the bands of equal temperature run north and south along the continental shores.

The extreme range of temperature in the surface waters of the ocean is from 28° to 95° F., and 84 per cent of the surface waters have a temperature exceeding 40° F. There is a circum-tropical zone where there is a high temperature and small range not exceeding 10° F., which embraces most of the coral-reef regions of the world, and there are two circum-polar zones where there is a low temperature and small range not exceeding 10° F., where carbonate-of-lime secreting organisms are poorly developed. Between these two polar zones and the circum-tropical zone are two intermediate zones where there is a wide range of temperature. It is in these intermediate zones that warm currents occupy the surface at one season of the year and cold currents at another season, and here there is a consequent great destruction of marine life. This gives us some indication of the conditions under which phosphatic and glauconitic deposits were laid down in past ages.

Many areas at the surface of the ocean used formerly to be regarded as barren and devoid of life, but there are no such barren regions. The whole surface of the ocean—both in cold and warm waters, and down to a depth of 600 feet—must be regarded as a vast meadow, more extensive

and more important than the vegetable covering on land-surfaces. Everywhere there are myriads of Diatoms, calcareous and other microscopic Algæ with a red-brown color, the chlorophyll in which is ever busy under the influence of the sun's rays converting inorganic into organic compounds. These minute organisms are the original source of food for the vast majority of marine animals both in the surface waters and on the floor of the ocean, even at the greatest depths. The reserve food of these minute organisms is little globules of oil, instead of granules of starch which prevail in terrestrial vegetation. This is doubtless the original source of the oil which appears in marine fishes, birds, and mammals in such abundance.

Many interesting physiological problems are suggested by the study of oceanography. In the ocean there are very few warm-blooded and air-breathing animals, and we have to deal chiefly with cold-blooded animals, the temperature of whose blood and bodies rises and falls with that of the water in which they live. In the tropics marine animals —for instance, a Copepod or Amphipod,—pass all their lives in water with a temperature of 80° to 90° F. In the polar seas a quite similar animal passes the whole of its life in water below the freezing-point. In these cases it is evident that the metabolism of the warm-water animal is much more rapid than that of the cold-water one; it reproduces its kind much more frequently, and its individual life is shorter than in the case of the cold-water animal. All chemical and all physiological changes take place much more rapidly in warm than in cold water. In cold sea-water there is much albuminoid ammonia, in warm water regions much saline ammonia, which fact points to more rapid change in the warm water of the ocean. By remembering these conditions we may account for the fact that genera and species are much more numerous in the warm water,

while on the other hand the species are few, but the individuals of a species are enormously greater, in the cold water. The animals in a tow-net from the tropics are most probably not more than a few weeks old, whereas a similar tow-net in the polar waters captures animals, some a few weeks old, and others, it may be, years of age. It seems certain that the warm tropical waters are the most favorable for vigorous life and rapid change, and here the struggle for life is most severe, and the evolution of new species much more frequent, than in the cold waters of the poles or the deep sea. In this direction we must look for an explanation of the so-called bipolarity in the distribution of marine organisms.

A great characteristic of organisms in warm tropical waters is the very large quantity of carbonate of lime they secrete from the ocean. This is evident, not only in the massive coral reefs, but also in the abundance of calcareous organisms in the plankton of the tropics—like coccospheres, Globigerinæ, and mollusks. All these lime-secreting organisms become less abundant as we approach the poles, or descend into the deep sea. In the warm water the carbonate of lime is deposited in shells and skeletons as aragonite, but in the cold water it is deposited much more slowly, and in the form of calcite. This shows that when we find a limestone rock with abundance of fossil-shells we may assume that it was laid down in a warm sea where the temperature approached 70° or 80° F. It may be safely asserted that at the present time lime is being accumulated towards the tropics through the action of lime-secreting organisms which obtain the lime from the sulphate of lime in sea-water.

The great abundance of pelagic larvæ of benthonic organisms in warm tropical surface waters, their periodicity in the intermediate zones with a wide range of temperature,

and their almost total absence in polar waters and in the deep sea, are facts in distribution of great interest to the biologist and evolutionist, and may be accounted for by the varied temperature conditions in the several areas.

The temperature conditions in the deep sea and on the floor of the ocean form a striking contrast to those prevailing in the surface waters. The lines of equal temperature, instead of running parallel to the equator, as at the surface, run on the whole north and south, following the general trend of the continents. The water which rests immediately on the ocean's floor in great depths has nearly everywhere a temperature under 40° F., and a very large part of it is below the freezing-point. Only a small band running north and south in shallow water along the continental shores has a temperature over 40° F. It follows, then, that much more than one half of the solid crust of our globe is kept at a low temperature at all times. The abysmal regions have not only a low temperature, but eternal darkness reigns there so far as the sun's rays are concerned; any motion which takes place in the water must be of extreme slowness. Transport and erosion do not take place in this deep region, which is an area of deposition. The materials composing the deposits, being saturated with sea-water during immense periods of time, become highly altered, and secondary products are formed in and on the surface of the deposits, such as manganese-iron nodules, palagonite, and zeolitic crystals.

The animals which have been able to accommodate themselves to life in the abysmal regions derive their food primarily from the dead organisms and excreta which have fallen from the surface waters; they are, indeed, mud-eaters. There is much reason to believe that the whole of the marine deposits are sooner or later eaten by organisms; it is, indeed, probable that all stratified rocks, whether ma-

rine or lacustrine, have in like manner passed through the intestines of animals. In many instances the excreta of the benthonic animals are converted into glauconitic and phosphatic grains. Phosphorescent light plays a large part in the economy of marine organisms, and it is a remarkable fact that this phenomenon of phosphorescence has never been observed in any fresh-water organisms. Some deep-sea animals are blind, some have very large eyes, some have highly developed tentacular organs. Some have complicated organs for the emission of light, some are many times larger than their shallow-water allies, while others are much smaller. All have a rather feeble development of calcareous shells and skeletons and a rather sombre color. All these modifications can be satisfactorily explained by reference to the pressure, the temperature, the food, the light, and other physical and biological conditions to which we have referred as prevailing in the deep water of the great ocean basins

A point of some interest to paleontologists is that in deep marine deposits the remains of marine organisms which lived on the bottom in cold water with a temperature below zero are mingled with the remains of surface organisms which lived at a temperature of 80° F.

It has been shown by hundreds of analyses of ocean-water from all parts of the world that the chemical composition of sea-water, that is to say, the ratio of acids to bases in sea-salts, is very constant, with some insignificant exceptions. Sea-water has acted as a gigantic solvent! it almost certainly now contains every known chemical element. The salts now present in solution represent what water has been able to leach and filter out of the solid crust and sea-water has been able to retain in solution. The history of the composition of sea-water should be the complement of all the terrestrial changes that have taken place

on the dry land of the continental areas. An endeavor may be now made to trace that history, in the same way that the geologist and paleontologist trace the evolution of the stratified rocks. We have now many indications that the composition of the sea-water salts—or rather, the proportion in them of the various elements—has continually changed from that of the primeval ocean.

It has been pointed out that Radiolaria, Diatoms, and other silica-secreting Protozoa and Protophyta, are more abundant where sea-water mixes with a large amount of fresh water in the present ocean, as, for instance, in the tropical West Pacific and in the Antarctic. When we remember the abundance of Radiolaria in Paleozoic schists, it seems to show that in the early seas there was much more detrital and colloid silicate of alumina in ocean waters than at the present time, the oceans being on the whole much shallower and less salt. Again, in the present seas lime-secreting organisms are much more abundant in the warmest and saltest waters than elsewhere. This indicates, when the small development of limestone in the earliest stratified formations is considered, that lime was less abundant in the pre-Cambrian ocean than in our seas. Indeed, water before the formation of soil on the land surfaces would carry to the ocean very different salts in solution from those carried at this time. Potassium, for instance, is absorbed at the present time by all soils, and the same element has from the earliest times been extracted from the sea-water to form glauconite. Potassium is, then, probably much less abundant now than in the primeval ocean. Lime has also been extracted in greater abundance in recent than in ancient seas. This occurs especially in the warmest and saltest seas.

Marine organisms have had to accommodate themselves to the slowly-changing conditions of the ocean which I

have just indicated, and it seems evident that the animal and vegetable protoplasm must have established fixed relations with the elements in solution in sea-water. This relation would almost certainly be handed on by heredity, for there is no reason for supposing that morphological structure can be handed down in this way, and not chemical composition of the soft tissues. When we have a fuller knowledge of the chemical composition of the soft tissues of the different groups of marine organisms, and of the composition of their circulatory fluids, we may possibly be able to read the history of the ocean as clearly as the paleontologist reads the history of the rocks.

THE PRESENT PROBLEMS OF METEOROLOGY

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Never in the history of the science have so many problems presented themselves for solution as at the present time. Numerous *a priori* theories require demonstration, and, in fact, the whole structure of meteorology, which has been erected on hypotheses, needs to be strengthened or rebuilt by experimental evidence. Until recently the observations have been carried on at the very bottom of the atmosphere, and our predecessors have been compared justly to shell-fish groping about the abysses of the ocean-floor to which they are confined.

Probably meteorology had its origin in a crude system of weather predictions, based on signs in the heavens, and it did not become a science until the invention of the principal meteorological instruments in the seventeenth century made possible the study of climatology by the collection of exact and comparable observations at many places on the globe. These data, owing to extensive operations of the meteorological services in the different countries, are now

tolerably complete, there being comparatively small portions of the land-surface, at least, for which the climatic elements are not fairly well known, the gaps that remain to be filled lying chiefly on the Antarctic continent and in the interior of Africa.

Although it is about fifty years ago since the first observations, made synchronously over a considerable territory, were telegraphed to a central office for the purpose of forecasting the weather, it must be confessed that practically no progress has been realized in this art, for, while much has been done to complete and extend the area under observation by the creation of a finer and wider network of stations, and while the transmission of the observations and the dissemination of the forecasts based on them have been accelerated, the methods employed in formulating the forecasts are essentially those empirical rules which were adopted at the inception of the work. A recent extension of the field of observation over the ocean, by wireless telegraphy, may here be mentioned as offering advantages to certain countries; for example, the reports now being received in England from steamers in mid-Atlantic give information about the approaching weather conditions,—subject, of course, to any subsequent changes,—long before they reach the western shores of the British Isles.[1] Nevertheless, the data obtained still relate mainly to the lowest strata of the atmosphere, and we are ignorant of the conditions that prevail at the height of a mile or two, both during storms and in fine weather. Until these are known, and their sequence in the upper and lower atmosphere has been established by careful investigation, our weather forecasts, based on synoptic observations, will continue to be largely empirical. However, it should be remembered that, since weather predictions constitute the aspect of meteor-

[1] *Nature*, vol. LXX, pp. 396-397.

ology which most appeals to mankind, the incentive to improve them is the most likely to stimulate the investigations needed. Therefore it is the problems of dynamic meteorology that now press for solution, and to achieve this purpose we must not only look upward, but also elevate ourselves, or our instruments, into the higher regions.

This mode of study belongs entirely to the last half-century, for only within that period has a systematic attempt been made to ascertain the conditions prevailing in the upper air. To the credit of the United States it should be remembered that the first post of observation upon a mountain peak was one established in 1871 upon Mount Washington in New Hampshire, and this was soon followed by the highest observatory in the world, maintained during fifteen years upon the summit of Pike's Peak in Colorado.[1] The observatory upon the Puy de Dôme in France, opened in 1876, was the first mountain station in Europe to be equipped with self-recording instruments.[2] A large amount of data has been collected at these stations which illustrate chiefly the climatology of the mountainous regions, for what we obtain in this way still pertains to the earth, and, as is now admitted, does not represent the conditions prevailing at an equal height in the free air. During the present century, the organized efforts which have been made to explore the ocean of air above us have already resulted in a great increase of knowledge respecting the atmosphere as a whole. This task of ascertaining the conditions of the free air was resumed in 1888 with balloon ascents in Germany, in which special precautions were taken to obtain accurate temperatures,[3] previous observations in balloons leaving much to be desired in this respect. Four years later the French demonstrated that by means

[1] *American Meteorological Journal*, vol. VIII, pp. 396-405.
[2] *Ibid.* vol. II, pp. 538-543.
[3] *Ibid.* vol. IX, pp. 245-251.

of balloons carrying only self-recording instruments, meteorological information might be acquired at heights far greater than those to which a human being can hope to ascend and live.[1] The use of the so-called *ballonssondes,* liberated and abandoned to their fate with the expectation that when they fall to the ground the records will be recovered, was soon adopted in Germany, and has since spread all over Europe. It has been introduced into the United States by the writer, who has just dispatched the first of these registration-balloons from St. Louis, hoping in this way to obtain the temperatures at heights never before reached above the American continent.[2]

In 1894, at the Blue Hill Observatory, near Boston, kites were first used to lift self-recording instruments and so obtain graphical records of the various meteorological elements in the free air,[3] and this method of observation, which presents the great advantage of securing the data in the different atmospheric strata almost simultaneously and nearly vertically above the station on the ground, has been extensively employed both in this country and abroad. Heights exceeding three miles have been attained, and it is possible to ascend a mile or two on almost any day when there is wind. To render the method independent of this factor, the plan of flying kites from a steamship was introduced by the writer three years ago,[4] and this scheme, too, is now being successfully employed in Europe. The exploration of the free air by balloons and kites, it may be remarked, has given rise to the construction of special types of light and simple self-recording instruments, which are capable of recording automatically the values of temperature, moisture, and wind with a precision comparable to

[1] *Nature,* vol. XLVIII, pp. 160-161.
[2] *Science,* N. S., vol. XXI, pp. 76-77.
[3] *Quarterly Journal of Royal Meteorological Society,* vol. XXIV, pp. 250-259.
[4] *Ibid.* vol. XXVIII, pp. 1-16.

the eye-readings of standard instruments by a good observer.

Having examined some of the newer methods of meteorological investigation, let us now consider how they may help to solve certain problems in dynamic meteorology. It should be premised that, since the atmosphere is relatively a thin layer with respect to the globe which it covers, no portion of it can be regarded as independent of another, and, consequently, a weather-map of the whole globe, day by day, is of prime importance. Were this provided, the atmospheric changes occurring simultaneously in both hemispheres could be watched and their relation to what have been called "the great centres of action" investigated.[1] Thanks to the increasing area covered by reports from the various weather services, the unmapped surface of the globe is being diminished, so that a complete picture of the state of the atmosphere each day over the land is gradually coming into view.

The mathematical application of the theory of a rotating sphere surrounded by a heated atmosphere to explain the circulation of the atmosphere as we find it, has not been satisfactory, owing to our lack of knowledge of the conditions of the upper air, as well as our ignorance concerning the physical properties of the atmosphere itself. To acquire the latter knowledge, research laboratories must be established at selected points, at both high and low levels, and as subjects of study there may be mentioned the determination of the amount of heat received from the sun and its secular variation, if any, the radiating and absorbing power of the air, the relation of pressure, density, and temperature, the chemical composition of the air, its ionization and radioactivity, and other investigations which have been

[1] *Report of International Meteorological Committee*, St. Petersburg, 1899, Appendix XI.

proposed by Professors Abbe and McAdie[1] in their pleas for the creation of such aërophysical laboratories. The observatory now under construction by the United States Weather Bureau on a mountain in Virginia will, it is hoped, enable some of these problems to receive the attention which they deserve.[2]

The average circulation of the lower atmosphere is now well known, by reason of the monumental work of Lieutenant Maury on the winds over the oceans, and from the mass of data since collected over oceans and continents through the meteorological organizations of the various countries. While, naturally, much less is known regarding the circulation of the upper air, a great deal has been ascertained from the observations of clouds that were instituted a few years ago in various parts of the world by an international commission. In order to insure that the same cloud should everywhere be called by the same name, it was necessary to instruct the observers by publishing a cloud-atlas,[3] containing pictures and descriptions of the typical forms of clouds which experience has shown to be identical all over the globe. Then, during one year which had been agreed upon, measurements of the direction of drift and the apparent velocity of the several cloud-types were made at many stations, and measurements, by trigonometrical or other methods, of the height of these clouds above a few selected stations enabled the true velocity of the air-currents to be determined up to the altitude at which the cirrus clouds float.[4] Thus an actual survey of the direction and speed of the atmospheric circulation at different levels was effected, and a recent discussion of the results by Professor Hildebrandsson shows that the theories which

[1] *Smithsonian Miscellaneous Collections,* vol. XXXIX, no. 1077.
[2] *National Geographic Magazine,* vol. XV, pp. 442-445.
[3] *Atlas International des Nuages,* Paris, 1896.
[4] *Quarterly Journal of Royal Meteorological Society,* vol. XXX, pp. 317-322.

have been held heretofore are untenable. Professor Hildebrandsson's conclusions in brief are that there is no exchange of air between poles and equator, the circulation over the oceans, at least, resolving itself into four great whirls, the air which rises above the tropics flowing over the trades and descending probably in the extra-tropical regions, while around each pole is an independent cyclonic circulation.[1] Although this general circulation of the atmosphere appears to be indicated, many details require to be investigated. In particular, the movements of the masses of air overlying the trade-winds and doldrums, which is a region nearly barren of upper clouds, are still unknown, and the determination of these movements, as well as the temperature and humidity of the different strata, by means of kites flown from steamships, was suggested by the writer, since it would be possible in this way to penetrate even the masses of quiescent air which probably separate the trade-winds from the superposed anti-trades.[2] This suggestion has already been put in practice on the yacht of the Prince of Monaco in the neighborhood of the Azores,[3] but a more extensive campaign is necessary, which the writer himself hopes to undertake, if the funds necessary to charter and equip a steamer can be procured.

Here it will be encouraging to state some results of the efforts to ascertain the vertical thermal and hygrometric gradients in the atmospheric ocean, and to show what may be accomplished in the future. Observations on mountains, as we have seen, cannot be expected to give the conditions which exist at the corresponding heights in the free air, and hence the necessity of sending observers or self-recording instruments into this medium through the agency of

[1] *Quarterly Journal of Royal Meteorological Society*, vol. xxx, pp. 322-343.

[2] *Monthly Weather Review* of United States Weather Bureau, vol. xxx, pp. 181-183.

[3] *Nature*, vol. LXXI, p. 467.

balloons and kites. By the aid of an international commission, formed eight years ago under the direction of Professor Hergesell at Strassburg, much has been accomplished in Europe in this way, and something in this country through kite-flights. At the present time such atmospheric soundings are made once a month in most European countries, and at Blue Hill in the United States, with the result that a knowledge is being acquired of the vertical gradients of the meteorological elements which entirely contradicts previous conceptions. For example, it was formerly supposed that the temperature diminished with increasing altitude more and more slowly, and that at a height of about ten miles it remained invariable during winter and summer and above pole and equator. But the recent investigations of my colleagues in France and Germany show that the temperature decreases faster and faster as one rises in the air, and that not only is there a large seasonal variation at the greatest heights attained, but that non-periodic changes occur from day to day, as they do at the earth's surface.[1] Still more remarkable is the indication of a warm current at a height of about seven miles, while the stratification of the atmosphere as regards temperature, moisture, and wind has been shown by the kite-flights at Blue Hill to be a normal condition, and not merely confined to the high atmosphere, as was formerly supposed. Daily soundings of the atmosphere to the height of a mile or two are now being made with kites or captive balloons at the meteorological institutes of Berlin, Hamburg, and St. Petersburg, and are furnishing valuable data concerning the changes in the meteorological elements which occur simultaneously or successively in the overlying strata.[2]

Of the various unsolved questions relating to this sub-

[1] *Monthly Weather Review*, vol. xxx, pp. 357-359.

[2] *Report of International Meteorological Committee*, Southport, 1903. Appendix II.

ject, perhaps the most important is whether the core of the cyclone possesses the excess of temperature over the surrounding body of air which the convectional theory of its origin requires. We need to know also the height to which the cyclone extends, the circulation around it at various levels, and further to generalize the theory of an accompanying cold-centre cyclone in the upper air, deduced by Mr. Clayton from the Blue Hill observations.[1] Other important questions which can be elucidated by future researches are the conditions favorable for precipitation and the action of dust-nuclei in producing it, the source of our American cold-waves, the exact relations of thunderstorms and tornadoes to centres of pressure and temperature, and, finally, the causes which, in the upper air, influence the trajectories and velocities of the cyclones and anti-cyclones that give us our broader weather features. When these correlations are determined from the investigations of the free air now in progress, and we possess a sufficient number of aërial stations to make it possible to chart a daily map of the upper air, then we may expect an improvement in the weather forecasts. The prediction of fog over the ocean on and adjacent to our coasts is of great practical importance to shipping, especially off the banks of Newfoundland, and the writer believes that meteorological kites flown from a steamer in these regions would reveal the unknown conditions of temperature, humidity, and wind in and above the fog-bank which might lead to the prediction of the situations favorable to its formation.

We now pass to another branch of meteorological research, namely, the cosmical relations. It is incontestable that the sun, the source of all terrestrial energy, has great influence upon the magnetic conditions of the earth, but a consideration of the relation of terrestrial magnetism and

[1] Blue Hill Meteorological Observatory, *Bulletin* No. 1, 1900.

meteorology will be left to my colleague, Dr. Bauer. The cause of atmospheric electricity has always been an enigma to meteorologists, but the discovery of "ions," or "elections," as carriers of electricity has thrown some light on this question.[1] It is of importance in geophysics to know how the capacity of the air for positive and negative electrons varies with altitude, to learn the periodic and non-periodic variation of the potential at the earth's surface and the law of dissipation of electricity.

Attempts to regard all atmospheric phenomena as periodic and due to the influence of the sun or moon have long occupied the attention of eminent investigators, but it must be admitted that the effects of neither the periods of solar nor of lunar rotation upon the earth's meteorology can be claimed to have been proved, although a correspondence has been found by the distinguished speaker who preceded me in regard to the frequencies of auroras and thunder-storms and the position of the moon in declination.[2] To Professor Arrhenius is also due the remarkable generalization that the pressure of light emanating from the sun causes alike the streaming-away from it of comets' tails, the zodiacal light, and the aurora borealis. The relation of sun-spot frequency, which has a periodicity of about eleven years, to atmospheric changes on the earth, especially as manifested by barometric pressure, rainfall, and temperature in India, has been investigated, and the coincidences, even if nothing more, which have been shown to exist by Sir Norman Lockyer and his son are suggestive.[3] It may be pointed out that the same action of the sun might cause simultaneously increased rain-fall in India and a deficiency of rainfall in England, because rising currents in one region are necessarily accompanied by descending currents else-

[1] *Terrestrial Magnetism and Atmospheric Electricity,* vol. VI, pp. 9-10.
[2] Arrhenius, *Lehrbuch der Kosmischen Physik,* pp. 791, 893.
[3] *Nature,* vol. LXIX, pp. 351-357.

where, and, therefore, no objection can be offered to a theory of cosmical influence which produces different weather conditions in different parts of the globe.

Since the sun is the source of our energy, the discovery of any variation in the heat emitted is of the deepest interest, and the important investigations of Professor Langley[1] are now to be supplemented by the broader work of a committee appointed by the National Academy of Sciences[2] and also by an international commission,[3] with the general object of combining and discussing meteorological observations from the point of view of their relation to solar phenomena. It does not seem improbable, therefore, that eventually we may have seasonal predictions of weather possessing at least the success of those now made daily, and that possibly forecasts of the weather will be hazarded several years in advance. The value of such forecasts, as effecting the crops alone, would be of inestimable benefit to mankind, and predictions already made in India for the ensuing season, while not entirely successful, have still proved advantageous. A number of short cycles in the weather have been detected, including a seven-day period in the temperature, which Mr. Clayton found could be used for forecasting were it not for an unexplained reversal in the phase of the temperature oscillation.[4]

The interesting question of the value of meteorological observations may appropriately conclude this address. Professor Schuster, the English physicist, has recently denounced the practice of accumulating these observations with no specific purpose.[5] To an extent this criticism is

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[1] *Report of Secretary of Smithsonian Institution*, 1903, pp. 23, 78-84.

[2] *Science*, N. S., vol. xx, pp. 316, 930-932.

[3] *Quarterly Journal of Royal Meteorological Society*, vol. xxxi, p. 28.

[4] *Proceedings of American Academy of Arts and Sciences*, vol. xxxiv, p. 613 *et seq.*

[5] Address at British Association, Belfast, 1902. *Nature*, vol. LXVI, pp. 617-618.

valid in all the sciences, since those observations are most useful when made by or for the person who is to utilize them, but although modern meteorology demands special series of observations to solve such problems as the temperature in cyclones and anti-cyclones, it is sometimes true that long series of observations made with one object in view may subsequently become valuable for quite another purpose. For the study of climate and its possible change long-continued observations in each country are a necessity, though these might properly be confined to selected stations from whose normals the values for other stations may be computed. Professor Schuster's wish to limit the number of observations implies that the existing series have been inadequately discussed, for the reason that it is easier to find observers than competent investigators. For this unfortunate condition the weather services of most countries are chiefly to blame, because, being burdened with the routine work of collecting climatological and synoptic data and formulating and promulgating weather forecasts, which is the public estimate of their entire duty, most Government meteorological organizations concentrate their energies and expenditures on these functions, and partially or completely neglect the researches by which alone our knowledge of the mechanics of the atmosphere can be increased. In this criticism must be included the United States Weather Bureau (exception being made in favor of Professor Bigelow's discussions), and the similar bureaus of such equally enlightened countries as France and England. However, in the latter country an attempt is now being made to create an Imperial meteorological institute which could undertake the discussion of the great mass of data accumulated in Great Britain and her colonies, especially the relations of solar phenomena to meteorology and magnetism, and it is argued that this would contribute towards the form-

ation of a body of scientific investigators adequate to the needs of the British Empire, and be of the highest educational and scientific worth.[1] In the United States, meteorological research has always been fostered by individuals, of whom the names of Franklin, Redfield, Espy, Coffin, Maury, Loomis, and Ferrel are brilliant examples. To-day my colleague, M. Teisserenc de Bort in France, and we ourselves at Blue Hill, are endeavoring, unassisted, to solve problems in dynamic meteorology, which ought to be undertaken by the national services of our respective countries. It behooves, then, those who are desirous of advancing the status of meteorology to strive to convince the public that the function of a Government Bureau is not merely to collect meteorological data and to make inductive weather predictions based on remembrance of the sequence in similar conditions, but that the science of meteorology requires laborious researches by competent men and the generous expenditure of money before practical benefit can result from improved weather forecasts. If some of my hearers are converted to such an opinion, this address will have served a useful purpose.

[1] Sir J. Eliot at British Association, Cambridge, 1904. *Nature*, vol. LXX, p. 406.

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THE PRESENT PROBLEMS OF METAPHYSICS

BY ALEXANDER T. ORMOND

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I.

The Preliminary Question

THE living problems of any science arise out of two sources: (1) out of what men may think of it, in view of its nature and claims, and (2) the problems that at any period are vital to it, and in the solution of which it realizes the purpose of its existence. Now if we distinguish the body of the sciences which deal with aspects of the world's phenomena—and here I would include both the psychic and the physical—from metaphysics, which professes to go behind the phenomenon and determine the world in terms of its inner, and, therefore, *ultimate* reality, it may be truly said of the body of the sciences that they are in a position to disregard in a great measure questions that arise out of the first source, inasmuch as the data from which they make their departure are obvious to common observation. Our world is all around us, and its phenomena either press upon us or are patent to our observation. Lying thus within the field of observation, it does not occur to the average mind to question either the legitimacy or the possibility of that effort of reflection which is devoted to their investigation and interpretation. Meta-

physics, however, enjoys no such immunity as this, but its claims are liable to be met with skepticism or denial at the outset, and this is due partly to the nature of its initial claims, and partly to the fact that its real data are less open to observation than are those of the sciences. I say partly to the nature of the initial claims of metaphysics, for it is characteristic of metaphysics that it refuses to regard the distinction between phenomena and ground or inner nature, on which the sciences rest, as final, and is committed from the outset to the claim that the real is in its inner nature one and to be interpreted in the light of, or in terms of, its inner unity; whereas, science has so indoctrinated the modern mind with the supposition that only the outer movements of things are open to knowledge, while their inner and real nature must forever remain inaccessible to our powers; I say that the modern mind has been so imbued with this pretension as to have almost completely forgotten the fact that the distinction of phenomenon and ground is one of science's own making. Neither the plain man nor the cultured man, if he happens not to be tinctured with science, finds his world a duality. The things he deals with are the realities, and it is only when his naïve realism begins to break down before the complex demands of his growing life, that the thought occurs to him that his world may be more complex than he has dreamed. It is clear, then, that the distinction of our world into phenomena and ground, on which science so largely rests, is a first product of reflection, and not a fact of observation at all.

If this be the case, it may be possible and even necessary for reflection at some stage to transcend this distinction. At least, there can be no reason except an arbitrary one for taking this first step of reflection to be a finality. And there would be the same justification for a second step that would transcend this dualism, as for the initial step out

of which the distinction arose; provided, it should be found that the initial distinction does not supply an adequate basis for a rational interpretation of the world that can be taken as final. Now, it is precisely because the dualistic distinction of the sciences does fail in this regard, that a further demand for a reflective transformation of the data arises. Let us bear in mind that the data of the sciences are not the simple facts of observation, but rather those facts transformed by an act of reflection by virtue of which they become phenomena distinguished from a more fundamental nature on which they depend and which itself is not open to observation. The real data of science are found only when the world of observation has been thus transformed by an act of reflection. If then at some stage in our effort to interpret our world it should become clear that the sciences of phenomena, whatever value their results may possess, are not given us an interpretation in terms that can be taken as final, and that in order to ground such an interpretation a further transformation of our data becomes necessary, I do not see why any of the sciences should feel that they have cause to demur. In truth, it is out of just such a situation as this that the metaphysical interpretation arises (as I propose very briefly here to show), a situation that supplies a genuine demand in the light of which the effort of metaphysics to understand its world seems to possess as high a claim to legitimacy as that of the sciences of phenomena. Let us take our stand with the plain man or the child, within the world of unmodified observation. The things of observation, in this world, are the realities, and at first we may suppose have undergone little reflective transformation. The first reflective effort to change this world in any way will, no doubt, be an effort to *number* or *count* the things that present themselves to observation, and out of this effort

will arise the transformation of the world that results from considering it under the concepts and categories of number. In short, to mathematical reflection of this simple sort, the things of observation will resolve themselves into a plurality of countable things, which the numbering reflection becoming explicit in its ordinal and cardinal moments will translate into a system that will be regarded as a whole made up of the sum of its parts. The very first step, then, in the reflective transformation of things resolves them into a dual system, the world conceived as a cardinal whole that is made up of its ordinal parts, and exactly equal to them. This mathematical conception is moreover purely quantitative; involving the exact and stable equivalence of its parts or units and that of the sum of the parts with the whole. Now it is with this purely quantitative transformation that mathematics and the mathematical sciences begin. We may ask, then, why should there be any other than mathematical science,[1] and what ground can non-mathematical science point to as substantiating its claims? I confess I can see no other final reason than this, that mathematical science does not meet the whole demand we feel obliged to make on our world. If mathematics were asked to vindicate itself, it no doubt would do so by claiming that things present quantitative aspects on which it founds its procedure. In like manner non-mathematical, or, as we may call it, physical or natural science, will seek to substantiate its claims by pointing to certain ultra-quantitative or qualitative aspects of things. It is true that, so far as things are merely *numerable,* they are purely quantitative; but mathematics abstracts from the content and character of its units and aggregates, which may and do change, so that a relation of stable equivalence is not maintained among them. In fact, the basis of these sciences is found in the

[1] I do not raise the question of qualitative mathematics at all. It is clear that the first mathematical reflection will be quantitative.

tendency of things to be always changing and becoming different from what they were before. The problem of these sciences is how to ground a rational scheme of knowledge in connection with a fickle world like that of qualitative change. It is here that reflection finds its problem, and noticing that the tendency of this world of change is for *a* to pass into *b* and thus to lose its own identity, the act of reflection that rationalizes the situation is one that connects *a* and *b* by relating them to a common ground *x* of which they stand as successive manifestations or symbols. *X* thus supplies the thread of identity that binds the two changes *a* and *b* into a relation to which the name causation may be applied. And just as quantitative equivalence is the principle of relationship among the parts of the simple mathematical world, so here in the world of the dynamic or natural sciences, the principle of relation is natural causation.[1] We find, then, that the non-mathematical sciences rest on a basis that is constituted by a *second act of reflection;* one that translates our world into a system of phenomena causally inter-related and connected with their underlying grounds.

We have now reached a point where it will be possible in a few sentences to indicate the rise of the metaphysical reflection and the ground on which it rests. If we consider both the mathematical and the physical ways of looking at things, we will find that they possess this feature in common,—they are purely external, having nothing to say respecting the *inner* and, therefore, *real* nature of the things with which they deal. Or, if we concede the latest claims of some of the physical speculators and agree that the aim of physics is an ultimate physical explanation of reality, it will still be true that the whole standpoint of this ex-

[1] By natural causation I mean such a relationship between *a* and *b* in a phenomenal system as enables *a* through its connection with its ground to determine *b*.

planation will be external. Let me explain briefly what I mean substantially by the term *external* as I use it here. Every interpretation of a world is a function of some knowing consciousness, and consequently of some knowing self. This is too obvious to need proof. A system will be *external* to such a knower just to the extent that the knower finds it dominated and determined by categories that are different from those of its own determination. A world physically interpreted is one that is brought completely under the rubrics of physics and mathematics; whose movements yield themselves completely, therefore, to a mechanical calculus that gives rise to purely descriptive formulæ; *or* to the control of a dynamic principle; that of natural causation, by virtue of which everything is determined without thought of its own, by the impulse of another, which impulse itself is not directly traceable to any thought or purpose. Now, the occasion for the metaphysical reflection arises when this situation that brings us face to face with, nay, makes us part and parcel of, an alien system of things, becomes intolerable, and the knower begins to demand a closer kinship with his world. The knower finds the categories of his own central and characteristic activity in experience. Here he is conscious of being an agent going out in forms of activity for the realization of his world. The determining categories of the activity he is most fully conscious of, are interest, idea, prevision, purpose, and that selective activity which goes to its termination in some achieved end. The metaphysical interpretation arises out of the demand that the world shall be brought into bonds of kinship with the knower. And this is effected by generalizing the categories of consciousness and applying them as principles of interpretation to the world. The act of reflection on which the metaphysical interpretation proceeds is one, then, in which the world of science is further

transformed by bringing the inner nature of things out of its isolation and translating the world-movements into process the terms of which are no longer *phenomena and hidden ground,* but rather inception and realization, or, more specifically, *Idea* and *Reality.* And the point to be noted here is the fact that these metaphysical categories are led up to positivity by an act of reflection that has for its guiding aim an interpretation of the world that will be more ultimately satisfactory to the knower than that of the physical or natural sciences; while negatively, it is led up to by the refusal of the knowing consciousness to rest in a world alien to its own nature and in which it is subordinated to the physical and made a mere epiphenomenon.

II.

Questions of Point of View, Principle and Method of Metaphysics

It is clear from what has been said that the metaphysical interpretation proceeds on a presupposition radically different from that of mathematical and physical science. The presumption of these sciences is that the world is physical, that the physical categories supply the norms of reality, and that consciousness and the psychic, in general, are subordinate and phenomenal to the physical. On the contrary, metaphysics arises out of a revolt from these presumptions toward the opposite presumption, namely, that *consciousness itself is the great reality,* and that the norms of an ultimate interpretation of things are to be sought in its categories. This is the great transformation that conditions the possibility and value of all metaphysics. It is the Copernican revolution which the mind must pass through, a revolution in which matter and the physical world yields the primacy to mind; a revolution in which consciousness

becomes central, its categories and analogies supplying the principles of final world-interpretation. Let us consider then, in the light of this great Copernican revolution, the questions of the *point of view, principle,* and *method* of metaphysics. And here the utmost brevity must be observed. If consciousness be the great reality, then its own central activity, that effort by which it realizes its world, will determine for us the *point of view* or departure of which we are in quest. This will be *inner* rather than *outer;* it will be motived by *interest,* will shape itself into interest-directed effort. This effort will be cognitive; dominated by an *idea* which will be an anticipation of the *goal* of the effort. It will, therefore, become *directive, selective,* and will stand as the *end* or *aim* of the completed effort. The whole movement will thus take the form, genetically, of a developing *purpose informed by an idea,* or *teleologically,* of a *purpose going on to its fulfillment* in some *aim* which is also its *motive.* Now, metaphysics determines its point of view in the following reasoning: if in consciousness we find the type of the inner nature of things, then the point of view for the interpretation of this inner nature will be to seek by generalizing the standpoint of consciously determined effort and asserting that this is the true point of view from which the *meaning* of the world is to be sought.

Having determined the metaphysical point of view, the next question of vital importance is that of its *principle.* And we may cut matters short here by saying at once that the principle we are seeking is that of *sufficient reason,* and we may say that a reason will be sufficient when it adequately expresses the world-view or concept under which an investigation is being prosecuted. Let us suppose that this world-view is that of simple mathematics, the principle of sufficient reason here will be that of *quantitative equivalence* of parts; or, from the standpoint of the whole,

that of *infinite divisibility*. Whereas, if we take the world of the ultra-mathematical science, which is determined by the notion of *phenomena depending on underlying ground,* we will find that the sufficient reason in this sphere takes the form of *adequate cause or condition.* The determining condition or causes of any physical phenomenon supply, from that point of view, the *ratio sufficiens* of its existence. We have seen that the sufficiency of a reason in the above cases has been determined in view of that notion which defines the kind of world the investigation is dealing with. Let us apply this insight to the problem of the principle of metaphysics, and we will soon conclude that no reason can be metaphysically sufficient that does not satisfy the requirements of a world conceived under the notion of *inception* and *realization;* or, more specifically, *idea* and *reality.* In short, the *reason* of metaphysics will refuse to regard its world as a mechanism that is devoid of thought and intention; that lacks, in short, the motives of internal determination and movement, and will in all cases insist that an explanation or interpretation can be metaphysically adequate only when its ultimate reference is to an idea that is in the process of *purposive* fulfillment. Such an explanation we call *teleological or rational,* rather than merely mechanical, and such a principle is alone adequate to embody the *ratio sufficiens* of metaphysics.

Having determined the point of view and principle of metaphysics, the question of metaphysical *method* will be divested of some of its greatest difficulties. It will be clear to any one who reflects that the very first problem in regard to the method of metaphysics will be that of its starting-point and the kind of results it is to look for. And little can be accomplished here until it has been settled that consciousness is to have the primacy, and that its prerogative is to supply both standpoint and principle of the investi-

gation. We have gone a long way toward mastering our method when we have settled these points: (1) that the metaphysical world is a world of consciousness; (2) that the conscious form of effort rather than the mechanical is the species of activity or movement with which we have to deal; and, (3) that the world it is seeking to interpret is ultimately one of *idea and reality* in which the processes take the *purposive form*. In view of this, the important steps of method (and we use the term method here in the most fundamental sense) will be (1) the question of the *form* of metaphysical activity or agency as contrasted with that of the physical sciences. This may be brought out in the contrast of the two terms *finality* and *mere efficiency*, in which by mere efficiency is meant an agency that is presumed to be thoughtless and purposeless, and consequently without *foresight*. All this is embodied in the term *force* or physical energy, and less explicitly in that of *natural causation*. Contrasted with this, *finality* is a term that involves the forward impulse of *idea, prevision,* and *purpose*. Anything that is capable of any sort of *foretaste* has in it a principle of prevision, selection, choice, and purpose. The impulse that motives and runs it, that also stands out as the *end* of its fulfillment, is a foretaste, an *Ahnung*, an anticipation, and the whole process or movement, as well as every part of it, will take on this character. (2) The second question of method will be that of the nature of this category of which *finality* is the form. What is its content, pure idea or pure will, or a synthesis that includes both? We have here the three alternatives of *pure rationalism, voluntarism,* and a doctrine hard to characterize in a single word; that rests on a *synthesis* of the norms of both rationalism and voluntarism. Without debating these alternatives, I propose here briefly to characterize the *synthetic* concept as supplying what I conceive to be the most satis-

factory doctrine. The principle of *pure rationalism* is one of insight but is lacking in practical energy, whereas, that of *voluntarism* supplies practical energy, but is lacking in insight. Pure voluntarism is *blind,* while pure rationalism is *powerless.* But the synthesis of *idea* and *will,* provided we go a step further (as I think we must) and presuppose also a germ of *feeling* as *interest,* supplies both *insight* and *energy.* So that the spring out of which our world is to arise may be described as either the *idea informed with purposive energy,* or *purpose or will informed and guided by the idea.* It makes no difference which form of conception we use. In either case if we include feeling as interest we are able to conceive movements originating in some species of apprehension, taking the dynamic form of purpose, and motived and selected, so to speak, by interest; and in describing such activity we are simply describing these normal movements of consciousness with which our experience makes us most familiar. (3) The third question of method involves the relation or correlation of the metaphysical interpretation with that of the natural or physical science. Two points are fundamental here. In the first place, it must be borne in mind that it is the same world with which the plain man, the man of science, and the metaphysician are concerned. We cannot partition off the external world to the plain man, the atoms and ethers to the man of science, leaving the metaphysician in exclusive and solitary possession of the world of consciousness. It is the same world for all. The metaphysician cannot shift the physical world, with its oceans and icebergs, its vast planetary systems and milky ways, on to the shoulders of the physicist. This is the metaphysician's own recalcitrant world, which will doubtless task all his resources to explain. In the *second* place, though it is the same world that is clamoring for interpretation, it is a world that passes

through successive transformations, in order to adapt itself to progressive modes of interpretation. The plain man is called to pass through a species of Copernican revolution that subordinates the phenomenon to its ground, before he can become a man of science. In turn, the man of science must go through the Copernican process, and learn to subordinate his atoms and ethers to consciousness before he can become a metaphysician. And it is this transformation that marks one of the most fundamental steps in the method of metaphysics. The world must experience this transformation, and it must become habitual to the thinker to subordinate the physical to the mental before the metaphysical point of view can be other than foreign to him. If, then, it be the same content with which the sciences and metaphysics are called on to deal, it is clear that we have on our hands another problem on the answer to which the fate of metaphysics vitally depends; the question of the *correlation* of its method with that of the sciences so that it may stand vindicated as the final interpretation of things.

III.

Question of the Correlation of Metaphysics With the Sciences

We have reached two conclusions that are vital here: (1) that the metaphysical way of looking at the world involves a transformation of the world of physical science; (2) that it is the same world that lies open to both science and metaphysics. Out of this arises the problem of the *correlation* of the two views; the two interpretations of the world. If science be right in conceiving the world under such categories as quantity and natural causation; if science be right in seeking a mechanical explanation of phenomena (that is, one that excludes prevision, purpose, and aim);

and if metaphysics be right in refusing to accept this explanation as final and in insisting that the principle of ultimate interpretation is teleological, that it falls under the categories of prevision, purpose, and aim; then it is clear that the problem of correlation is on our hands. In dealing with this problem, it will be convenient to separate it into two questions: (1) that of the fact; (2) that of its rationale. The fact of the correlation is a thing of common experience. We have but to consider the way in which this Congress of Science has been brought about in order to have an exhibition of the method of correlation. Originating first in the sphere of thought and purpose, the design has been actualized through the operation of mechanical agencies which it has somehow contributed to liberate. On the scale of individual experience we have the classic instance of the arm moving through space in obedience to a hidden will. There can be no question as to the fact and the great difficulty of metaphysics does not arise in the task of generalizing the fact and conceiving the world as a system of thought-purposes working out into forms of the actual through mechanical agencies. This generalization somehow lies at the foundation of all metaphysical faith, and, this being the case, the real task here, aside from the profounder question of the *rationale,* is that of exhibiting the actual points of correlation; those points in the various stages of the sciences from physics to ethics and religion, at which the last category or result of science is found to hold as its immediate implication some first term of the more ultimate construction of metaphysics. The working out of this task is of the utmost importance, inasmuch as it makes clear to both the man of science and the metaphysician the intrinsic necessity of the correlation. It is a task analogous to the Kantian deduction of the categories.

IV

Questions of the Ultimate Nature of Reality

We come, then, to the question of the rationale of this correlation, and it is clear here that we are dealing with a phase of the problem of the ultimate nature of reality. For the question of the correlation now is how it is possible that our thoughts should affect things so that they move in response; how mind influences body or the reverse, how, when we will, the arm moves through space. And without going into details of discussion here, let us say at once, that whatever the situation may be for any science,—and it may be that some form of *dualism* is a necessary presupposition of science,—for metaphysics it is clear that no dualism of substances or orders can be regarded as final. The life of metaphysics depends on finding the one for the many; the one that when found will also ground the many. If, then, the phenomenon of *mind and body* presents the appearance of a correspondence of two different and, so far as can be determined, mutually exclusive agencies, the problem of metaphysics is the reduction of these agencies to one species. Here we come upon the issue between materialism and immaterialism. But inasmuch as the notion of metaphysics itself seems to exclude materialism, the vital alternative is that of immaterialism. Again, if psychophysics presents as its basal category a *parallelism* between two orders of phenomena, psychic and physical, it is the business of metaphysics to seek the explanation of this dualism in some more ultimate and unitary conception. Now, since the very notion of metaphysics again excludes the physical alternative from the category of finality, we are left with the psychic term as the one that, by virtue of the fact that it embodies a form of *conscious* activity, promises to be most fruitful for metaphysics. From one

point of view, then, we have reduced our world to immaterialism; from another, to some form or analogue of the psychic. Now it is not necessary here to carry the inquiry further in this direction. For what metaphysics is interested in, specially, is the fact that the world must be reduced to one kind of being and one type of agency. If this be done, it is clear that the dualism of *body and mind* and the *parallel orders* of psycho-physics cannot be regarded as final, but must take their places as phenomena that are relative and reducible to a more fundamental unity. The metaphysician will say that the arm moves through space in response to the will, and that everywhere the correlation between mechanical and teleological agency takes place because in the last analysis *there is only one type of agency;* an agency that finds its initiative in interest, thought, purpose, design, and thus works out its results in the fields of space and mechanical activities.

Furthermore, on the question to which these considerations lead up; that of the ultimate interpretation we are to put on the reality of the world, the issue is not so indeterminate as it might seem from some points of view. Taking it that the very notion of metaphysics excludes the material and the physical as ultimate types of the real, we are left with the notions of the immaterial and the psychic; and while the former is indefinite, it is a fact that in the psychic and especially in the form of it which man realizes in his own experience, he finds an intelligible type and the only one that is available to him for the definition of the immaterial. He has his choice, then, either to regard the world as *absolutely opaque,* showing nothing but its phenomenal dress which ceases to have any meaning; or to apply to the world's inner nature the intelligible types and analogies of his own form of being. That this *is* the alternative that is embodied in the existence of meta-

physics is clearly demonstrated by the fact that the metaphysical interpretation embodies itself in the categories of *reason, design, purpose,* and *aim.* Whatever difficulties we may encounter, then, in the *use* and application of the *psychic analogy* in determining the nature of the real, it is clear that its employment is inevitable and indispensable. Let us, then, employ the term *rational* to that characterization of the nature of things which to metaphysics is thus inevitable and indispensable. The world must in the last analysis be *rational* in its constitution, and its agencies and forms of being must be construed as *rational* in their type.

And here we come upon the last question in this field, that of the *ultimate being of the world.* We have already concluded that the *real* is in the last analysis rational. But we have not answered the question whether there shall be one rational or many. Now it has become clear that with metaphysics *unity* is a cardinal interest; that, therefore, the world must be *one* in *thought, purpose, aim.* And it is on this insight that the metaphysical doctrine of the *absolute* rests. There must be *one* being whose thought and purpose are all-inclusive, in order that the world may be one and that it may have meaning as a whole. But the world presents itself as a plurality of finite *existents* which our metaphysics requires us to reduce in the last analysis to the psychic type. What of this plurality of psychic existents? It is on this basis that metaphysics constructs its doctrine of *individuality.* Allowing for latitude of opinion here, the trend of metaphysical reflection sets strongly toward a doctrine of reality that grounds the world in an Absolute whose all-comprehending thought and purpose utters or realizes itself in the plurality of finite individuals that constitutes the world; the degree of reality that shall be ascribed to the plurality of individuals being a point in debate, giving rise to the contemporary form of the issue

between idealism and realism. Allowing for minor differences, however, there is among metaphysicians a fair degree of assent to the doctrine that in order to be completely rational the world of individual plurality must be regarded as implying an *Absolute,* which, whether it is to be conceived as an individual or not, is the author and bearer of the thought and design of the world as a whole.

V

Questions of Metaphysical Knowledge and Ultimate Criteria of Truth

We have only time to speak very briefly, in conclusion, of two vital problems in metaphysics: (1) that of the nature and limits of metaphysical knowledge; (2) that of the ultimate criteria of truth. In regard to the question of knowledge, we may either *identify thought with reality,* or we may regard thought as *wholly inadequate to represent the real;* in one case we will be *gnostic,* in the other *agnostic.* Now whatever may be urged in favor of the gnostic alternative, it remains true that *our* thought, in order to follow along intelligible lines, must be guided by the categories and analogies of our own experience. This fixes a limit, so that the thought of man is never in a position to grasp the real completely. Again, whatever may be urged in behalf of the agnostic alternative, it is to be borne in mind that our experience does supply us with intelligible types and categories; and that under the impulse of the *infinite* and *absolute,* or the transcendent, to which our thought responds (to put it no stronger), a dialectical activity arises; on the one hand, the application of the experience-analogies to determine the real; on the other, the incessant removal of limits by the impulse of transcendence (as we may call it). Thus arises a *movement of ap-*

proximation which while it never completely compasses its goal, yet proceeds along intelligent lines; constitutes the mind's effort to know; and results in an *approximating series of intelligible and relatively adequate conceptions.* Metaphysically, we are ever approximating to ultimate knowledge; though it can never be said that we have attained it. The type of metaphysical knowledge cannot be characterized, therefore, as either gnostic or agnostic.

As to the question of ultimate *criteria,* it is clear that we are here touching one of the living issues of our present-day thought. Shall the judgment of truth, on which certitude must found, exclude practical considerations of value, or shall the consideration of value have weight in the balance of certitude? On this issue we have at the opposite extremes (1) the *pure rationalist* who insists on the rigid exclusion from the epistemological scale of every consideration except that of pure logic. The truth of a thing, he urges, is always a purely logical consideration. On the other hand, we have (2) the *pure pragmatist,* who insists on the *"will to believe"* as a legitimate datum or factor in the determination of certitude. The pragmatic platform has two planks: (1) the *ontological*—we select our world that we call real at the behest of our interests; (2) the *ethical*—in such a world practical interest has the right of way in determining what we are to accept as true as well as what we are to choose as good. It is my purpose in thus outlining the extremes of doctrine to close with a suggestion or two toward less ultra-conclusions. It is a sufficient criticism on the *pure rationalist's* position to point out the fact that his separation of practical and theoretic interests is a pure fiction that is never realized anywhere. The motives of science and the motives of practice are so blended that interest in the conclusion always enters as a factor in the process. A conclusion reached by the pure ra-

tionalists' methods would be one that would only interest the pure rationalist in so far as he could divest himself of all motives except the bare love of fact for its own sake. The *pure pragmatist* is, I think, still more vulnerable. He must, to start with, be a pure subjective idealist, otherwise he would find his world at many points recalcitrant to his ontology. Furthermore, the mere *will to believe* is arbitrary and involves the suppression of reason. In order that the will to believe may work *real* conviction, the point believed must at least amount to a postulate of the practical reason; it must become somehow evident that the refusal to believe would create a situation that would be theoretically unsound or irrational; as, for instance, if we assume that the immortality of the soul is a *real postulate* of practical reason, it must be so because the negative of it would involve the irrationality of our world; and therefore a degree of theoretic imperfection or confusion. Personally I believe the lines here converge in such a way that the ideal of truth will always be found to have practical value; and *conversely*, as to practical ideals, that a sound practical postulate will have weight in the theoretic scales. And it is doubtless true, as Professor Royce urges in his presidential address on *The Eternal and The Practical*, that all judgments must find their final warrant at the Court of the Eternal where so far as we can see, the theoretical and practical coalesce into one.

THE FIELD OF LOGIC

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CURRENT tendencies in logical theory make a determination of the field of logic fundamental to any statement of the general problems of the science. In view of this fact, I propose in this paper to attempt such a determination by a general discussion of the relation of logic to mathematics, psychology, and biology, especially noting in connection with biology the tendency known as pragmatism. In conclusion, I shall indicate what the resulting general problems appear to be.

I

There may appear, at first, little to distinguish mathematics in its most abstract, formal, and symbolic type from logic. Indeed, mathematics as the universal method of all knowledge has been the ideal of many philosophers, and its right to be such has been claimed of late with renewed force. The recent notable advances in the science have done much to make this claim plausible. A logician, a non-mathematical one might be tempted to say that, in so far as mathematics is the method of thought in general, it has ceased to me mathematics; but, I suppose, one ought

not to quarrel too much with a definition, but should let mathematics mean knowledge simply, if the mathematicians wish it. I shali not, therefore, enter the controversy regarding the proper limits of mathematical inquiry. I wish to note, however, a tendency in the identification of logic and mathematics which seems to me to be inconsistent with the real significance of knowledge. I refer to the exaltation of the freedom of thought in the construction of conceptions, definitions, and hypotheses.

The assertion that mathematics is a "pure" science is often taken to mean that it is in no way dependent on experience in the construction of its basal concepts. The space with which geometry deals may be Euclidean or not, as we please; it may be the real space of experience or not; the properties of it and the conclusions reached about it may hold in the real world or they may not; for the mind is free to construct its conception and definition of space in accordance with its own aims. Whether geometry is to be ultimately a science of this type must be left, I suppose, for the mathematicians to decide. A logician may suggest, however, that the propriety of calling all these conceptions "space" is not as clear as it ought to be. Still further, there seems to underlie all arbitrary spaces, as their foundation, a good deal of the solid material of empirical knowledge, gained by human beings through contact with an environing world, the environing character of which seems to be quite independent of the freedom of their thought. However that may be, it is evident, I think, that the generalization of the principle involved in this idea of the freedom of thought in framing its conception of space, would, if extended to logic, give us a science of knowledge which would have no necessary relation to the real things of experience, although these are the things with which all concrete knowledge is most evidently concerned. It

would inform us about the conclusions which necessarily follow from accepted conceptions, but it could not inform us in any way about the real truth of these conclusions. It would, thus, always leave a gap between our knowledge and its objects which logic itself would be quite impotent to close. Truth would thus become an entirely extra-logical matter. So far as the science of knowledge is concerned, it would be an accident if knowledge fitted the world to which it refers. Such a conception of the science of knowledge is not the property of a few mathematicians exclusively, although they have, perhaps, done more than others to give it its present revived vitality. It is the classic doctrine that logic is the science of thought as thought, meaning thereby thought in independence of any specific object whatever.

In regard to this doctrine, I would not even admit that such a science of knowledge is possible. You cannot, by a process of generalization or free construction, rid thought of connection with objects; and there is no such thing as a general content or as content-in-general. Generalization simply reduces the richness of content and, consequently, of implication. It deals with concrete subject-matter as much and as directly as if the content were individual and specialized. "Things equal to the same thing are equal to each other," is a truth, not about thought, but about things. The conclusions about a fourth dimension follow, not from the fact that we have thought of one, but from the conception about it which we have framed. Neither generalization nor free construction can reveal the operations of thought in transcendental independence.

It may be urged, however, that nothing of this sort was ever claimed. The bondage of thought to content must be admitted, but generalization and free construction, just because they give us the power to vary conditions as we

please, give us thinking in a relative independence of content, and thus show us how thought operates irrespective of, although not independent of, its content. The binomial theorem operates irrespective of the values substituted for its symbols. But I can find no gain in this restatement of the position. It is true, in a sense, that we may determine the way thought operates irrespective of any specific content by the processes of generalization and free construction; but it is important to know in what sense. Can we claim that such irrespective operation means that we have discovered certain logical constants, which now stand out as the distinctive tools of thought? Or does it rather mean that this process of varying the content of thought as we please reveals certain real constants, certain ultimate characters of reality, which no amount of generalization or free construction can possibly after? The second alternative seems to me to be the correct one. Whether it is or not may be left here undecided. What I wish to emphasize is the fact that the decision is one of the things of vital interest for logic, and properly belongs in that science. Clearly, we can never know the significance of ultimate constants for our thinking until we know what their real character is. To determine that character we must most certainly pass out of the realm of generalization and free construction; logic must become other than simply mathematical or symbolic.

There is another sense in which the determination of the operations of thought irrespective of its specific content is interpreted in connection with the exaltation of generalization and free construction. Knowledge, it is said, is solely a matter of implication, and logic, therefore, is the science of implication simply. If this is so, it would appear possible to develop the whole doctrine of implication by the use of symbols, and thus free the doctrine from dependence

on the question as to how far these symbols are themselves related to the real things of the world. If, for instance, *a* implies *b*, then, if *a* is true, *b* is true, and this quite irrespective of the real truth of *a* or *b*. It is to be urged, however, in opposition to this view, that knowledge is concerned ultimately only with the real truth of *a* and *b*, and that the implication is of no significance whatever apart from this truth. There is no virtue in the mere implication. Still further, the supposition that there can be a doctrine of implication, simply, seems to be based on a misconception. For even so-called formal implication gets its significance only on the supposed truth of the terms with which it deals. We suppose that *a does* imply *b*, and that *a is* true. In other words, we can state this law of implication only as we first have valid instances of it given in specific, concrete cases. The law is a generalization and nothing more. The formal statement gives only an apparent freedom from experience. Moreover, there is no reason for saying that *a* implies *b* unless it does so either really or by supposition. If *a* really implies *b*, then the implication is clearly not a matter of thinking it; and to suppose the implication is to feign a reality, the implications of which are equally free from the processes by which they are thought. Ultimately, therefore, logic must take account of real implications. We cannot avoid this through the use of a symbolism which virtually implies them. Implication can have a logical character only because it has first a metaphysical one.

The supposition underlying the conception of logic I have been examining is, itself, open to doubt and seriously questioned. That supposition was the so-called freedom of thought. The argument has already shown that there is certainly a very definite limit to this freedom, even when logic is conceived in a very abstract and formal way. The processes of knowledge are bound up with their contents,

and have their character largely determined thereby. When, moreover, we view knowledge in its genesis, when we take into consideration the contributions which psychology and biology have made to our general view of what knowledge is, we seem forced to conclude that the conceptions which we frame are very far from being our own free creations. They have, on the contrary, been laboriously worked out through the same processes of successful adaptation which have resulted in other products. Knowledge has grown up in connection with the unfolding processes of reality, and has, by no means, freely played over its surface. That is why even the most abstract of all mathematics is yet grounded in the evolution of human experience.

In the remaining parts of this paper, I shall discuss further the claims of psychology and biology. The conclusion I would draw here is that the field of logic cannot be restricted to a realm where the operations of thought are supposed to move freely, independent or irrespective of their contents and the objects of a real world; and that mathematics, instead of giving us any support for the supposition that it can, carries us, by the processes of symbolization and formal implication, to recognize that logic must ultimately find its field where implications are real, independent of the processes by which they are thought, and irrespective of the conceptions we choose to frame.

II.

The processes involved in the acquisition and systematization of knowledge may, undoubtedly, be regarded as mental processes and fall thus within the province of psychology. It may be claimed, therefore, that every logical process is also a psychological one. The important question is, however, is it nothing more? Do its logical and psychological

characters simply coincide? Or, to put the question in still another form, as a psychological process simply, does it also serve as a logical one? The answers to these questions can be determined only by first nothing what psychology can say about it as a mental process.

In the first place, psychology can analyze it, and so determine its elements and their connections. It can thus distinguish it from all other mental processes by pointing out its unique elements or their unique and characteristic connection. No one will deny that a judgment is different from an emotion, or that an act of reasoning is different from a volition; and no one will claim that these differences are entirely beyond the psychologist's power to ascertain accurately and precisely. Still further, it appears possible for him to determine with the same accuracy and precision the distinction in content and connection between processes which are true and those which are false. For, as mental processes, it is natural to suppose that they contain distinct differences of character which are ascertainable. The states of mind called belief, certainty, conviction, correctness, truth, are thus, doubtless, all distinguishable as mental states. It may be admitted, therefore, that there can be a thoroughgoing psychology of logical processes.

Yet it is quite evident to me that the characterization of a mental process as logical is not a psychological characterization. In fact, I think it may be claimed that the characterization of any mental process in a specific way, say as an emotion, is extra-psychological. Judgments and inferences are, in short, not judgments and inferences because they admit of psychological analysis and explanation, any more than space is space because the perception of it can be worked out by genetic psychology. In other words, knowledge is first *knowledge,* and only later a set of processes for psychological analysis. That is why, as it seems

to me, all psychological logicians, from Locke to our own day, have signally failed in dealing with the problem of knowledge. The attempt to construct knowledge out of mental states, the relations between ideas, and the relation of ideas to things, has been, as I read the history, decidedly without profit. Confusion and divergent opinion have resulted instead of agreement and confidence. On precisely the same psychological foundation, we have such divergent views of knowledge as idealism, phenomenalism, and agnosticism, with many other strange mixtures of logic, psychology, and metaphysics. The lesson of these perplexing theories seems to be that logic, as logic, must be divorced from psychology.

It is also of importance to note, in this connection, that the determination of a process as mental and as thus falling within the domain of psychology strictly, has by no means been worked out to the general satisfaction of psychologists themselves. Recent literature abounds in elaborate discussion of the distinction between what is a mental fact and what not, with a prevailing tendency to draw the remarkable conclusion that all facts are somehow mental or experienced facts. The situation would be worse for psychology than it is, if that vigorous science had not learned from other sciences the valuable knack of isolating concrete problems and attacking them directly, without the burden of previous logical or metaphysical speculation. Thus knowledge, which is the peculiar province of logic, is increased, while we wait for the acceptable definition of a mental fact. But definitions, be it remembered, are themselves logical matters. Indeed, some psychologists have gone so far as to claim that the distinction of a fact as mental is a purely logical distinction. This is significant as indicating that the time has not yet come for the identification of logic and psychology.

In refreshingly sharp contrast to the vagueness and uncertainty which beset the definition of a mental fact are the palpable concreteness and definiteness of knowledge itself. Every science, even history and philosophy, are instances of it. What constitutes a knowledge ought to be as definite and precise a question as could be asked. That logic has made no more progress that it has in the answer to it appears to be due to the fact that it has not sufficiently grasped the significance of its own simplicity. Knowledge has been the important business of thinking man, and he ought to be able to tell what he does in order to know, as readily as he tells what he does in order to build a house. And that is why the Aristotelian logic has held its own so long. In that logic, "the master of them that know" simply rehearsed the way he had systematized his own stores of knowledge. Naturally we, so far as we have followed his methods, have had practically nothing to add. In our efforts to improve on him, we have too often left the right way and followed the impossible method inaugurated by Locke. Had we examined with greater persistence our own methods of making science, we should have profited more. The introduction of psychology, instead of helping the situation, only confuses it.

Let it be granted, however, in spite of the vagueness of what is meant by a mental fact, that logical processes are also mental processes. This fact has, as I have already suggested, an important bearing on their genesis, and sets very definite limits to the freedom of thought in creating. It is not, however, as mental processes that they have the value of knowledge. A mental process which is knowledge purports to be connected with something other than itself, something which may not be a mental process at all. This connection should be investigated, but the investigation of it belongs, not to psychology, but to logic.

I am well aware that this conclusion runs counter to some metaphysical doctrines, and especially to idealism in all its forms, with the epistemologies based thereon. It is, of course, impossible here to defend my position by an elaborate analysis of these metaphysical systems. But I will say this, I am in entire agreement with idealism in its claim that questions of knowledge and of the nature of reality cannot ultimately be separated, because we can know reality only *as* we know it. But the general question as to how we know reality can still be raised. By this I do not mean the question, how is it possible for us to have knowledge at all, or how it is possible for reality to be known at all, but how, as a matter of fact, we actually do know it? That we really do know it, I would most emphatically claim. Still further, I would claim that what we know about it is determined, not by the fact that we can know in general, but by the way reality, as distinct from our knowledge, has determined. These ways appear to me to be ascertainable, and form, thus, undoubtedly, a section of metaphysics. But the metaphysics will naturally be realistic rather than idealistic.

III

Just as logical processes may be regarded as, at the same time, psychological processes, so they may be regarded, with equal right, as vital processes, coming thus under the categories of evolution. The tendency so to regard them is very marked at the present day, especially in France and in this country. In France, the movement has perhaps received the clearer definition. In America the union of logic and biology is complicated—and at times even lost sight of—by emphasis on the idea of evolution generally. It is not my intention to trace the history of this movement, but I should like to call attention to its historic motive in order to get it in a clear light.

That the theory of evolution, even Darwinism itself, has radically transformed our historical, scientific, and philosophical methods, is quite evident. Add to this the influence of the Hegelian philosophy, with its own doctrine of development, and one finds the causes of the rather striking unanimity which is discoverable in many ways between Hegelian idealists, on the one hand, and philosophers of evolution of Spencer's type, on the other. Although two men would, perhaps, not appear more radically different at first sight than Hegel and Spencer, I am inclined to believe that we shall come to recognize more and more in them an identity of philosophical conception. The pragmatism of the day is a striking confirmation of this opinion, for it is often the expression of Hegelian ideas in Darwinian and Spencerian terminology. The claims of idealism and of evolutionary science and philosophy have thus sought reconciliation. Logic has been, naturally, the last of the sciences to yield to evolutionary and genetic treatment. It could not escape long, especially when the idea of evolution had been so successful in its handling of ethics. If morality can be brought under the categories of evolution, why not thinking also? In answer to that question we have the theory that thinking is an adaptation, judgment is instrumental. But I would not leave the impression that this is true of pragmatism alone, or that it has been developed only through pragmatic tendencies. It is naturally the result also of the extension of biological philosophy. In the biological conception of logic we have, then, an interesting coincidence in the results of tendencies differing widely in their genesis.

It would be hazardous to deny, without any qualifications, the importance of genetic considerations. Indeed, the fact that evolution in the hands of a thinker like Huxley, for instance, should make consciousness and thinking ap-

parently useless epiphenomena in a developing world, has seemed like a most contradictory evolutionary philosophy. It was difficult to make consciousness a real function in development so long as it was regarded as only cognitive in character. Evolutionary philosophy, coupled with physics, had built up a sort of closed system with which consciousness could not interfere, but which it could know, and know with all the assurance of a traditional logic. If, however, we were to be consistent evolutionists, we could not abide by such a remarkable result. The whole process of thinking must be brought within evolution, so that knowledge, even the knowledge of the evolutionary hypothesis itself, must appear as an instance of adaptation. In order to do this, however, consciousness must not be conceived as only cognitive. Judgment, the core of logical processes, must be regarded as an instrument and as a mode of adaptation.

The desire for completeness and consistency in an evolutionary philosophy is not the only thing which makes the denial of genetic considerations hazardous. Strictly biological considerations furnish reasons of equal weight for caution. For instance, one will hardly deny that the whole sensory apparatus is a striking instance of adaptation. Our perceptions of the world would thus appear to be determined by this adaptation, to be instances of adjustment. They might conceivably have been different, and in the case of many other creatures, the perceptions of the world are undoubtedly different. All our logical processes, referring ultimately as they do to our perceptions, would thus appear finally to depend on the adaptation exhibited in the development of our sensory apparatus. So-called laws of thought would seem to be but abstract statements of formulations of the results of this adjustment. It would be absurd to suppose that a man thinks in a sense radically different from that in which he digests, or a flower blossoms,

or that two and two are four in a sense radically different from that in which a flower has a given number of petals. Thinking, like digesting and blossoming, is an effect, a product, possibly a structure.

I am not at all interested in denying the force of these considerations. They have, to my mind, the greatest importance, and due weight has, as yet, not been given to them. To one at all committed to a unitary and evolutionary view of the world, it must indeed seem strange if thinking itself should not be the result of evolution, or that, in thinking, parts of the world had not become adjusted in a new way. But while I am ready to admit this, I am by no means ready to admit some of the conclusions for logic and metaphysics which are often drawn from the admission. Just because thought, as a product of evolution, is functional and judgment instrumental, it by no means follows that logic is but a branch of biology, or that knowledge of the world is but a temporary adjustment, which, as knowledge, might have been radically different. In these conclusions, often drawn with Protagorean assurance, two considerations of crucial importance seem to be overlooked, first, that adaptation is itself metaphysical in character, and secondly, that while knowledge may be functional and judgment instrumental, the character of the functioning has the character of knowledge, which sets it off sharply from all other functions.

It seems strange to me that the admission that knowledge is a matter of adaptation, and thus a relative matter, should, in these days, be regarded as in any way destroying the claims of knowledge to metaphysical certainty. Yet, somehow, the opinion widely prevails that the doctrine of relativity necessarily involves the surrender of anything like absolute truth. "All our knowledge is relative, and, therefore, only partial, incomplete, and but practically trust-

worthy," is a statement repeatedly made. The fact that, if our development had been different, our knowledge would have been different, is taken to involve the conclusion that our knowledge cannot possibly disclose the real constitution of things, that it is essentially conditional, that it is only a mental device for getting results, that any other system of knowledge which would get results equally well would be equally true; in short, that there can be no such thing as metaphysical or epistemological truth. These conclusions do indeed seem strange, and especially strange on the basis of evolution. For while the evolutionary process might, conceivably, have been different, its results are, in any case, the results of the process. They are not arbitrary. We might have digested without stomachs, but the fact that we use stomachs in this important process ought not to free us from metaphysical respect for the organ. As M. Rey suggests, in the *Revue Philosophique* for June, 1904, a creature without the sense of smell would have no geometry, but that does not make geometry essentially hypothetical, a mere mental construction; for *we* have geometry because of the working out of nature's laws. Indeed, instead of issuing in a relativistic metaphysics of knowledge, the doctrine of relativity should issue in the recognition of the finality of knowledge in every case of ascertainably complete adaptation. In other words, adaptation is itself metaphysical in character. Adjustment is always adjustment between things, and yields only what it does yield. The things or elements get into the state which is their adjustment, and this adjustment purports to be their actual and unequivocal ordering in relation to one another. Different conditions might have produced a different ordering, but, again, this ordering would be equally actual and unequivocal, equally the *one* ordering to issue from them. To suppose or admit that the course of events might have

been and might be different is not at all to suppose or admit that it was or is different; it is, rather, to suppose and admit that we have real knowledge of what that course really was and is. This seems to be very obvious.

Yet the evolutionist often thinks that he is not a metaphysician, even when he brings all his conceptions systematically under the conception of evolution. This must be due to some temporary lack of clearness. If evolution is not a metaphysical doctrine when extended to apply to all science, all morality, all logic, in short, all things, then it is quite meaningless for evolutionists to pronounce a metaphysical sentence on logical processes. But if evolution is a metaphysics, then its sentence is metaphysical, and in every case of adjustment or adaptation we have a revelation of the nature of reality in a definite and unequivocal form. This conclusion applies to logical processes as well as to others. The recognition that they are vital processes can, therefore, have little significance for these processes in their distinctive character as logical. They are like all other vital processes in that they are vital and subject to evolution. They are unlike all others in that thought is unlike digestion or breathing. To regard logical processes as vital processes does not in any way, therefore, invalidate them as logical processes or make it superfluous to consider their claim to give us real knowledge of a real world. Indeed, it makes such a consideration more necessary and important.

A second consideration overlooked by the Protagorean tendencies of the day is that judgment, even if it is instrumental, purports to give us knowledge, that is it claims to reveal what is independent of the judging process. Perhaps I ought not to say that this consideration is overlooked, but rather that it is denied significance. It is even denied to be essential to judgment. It is claimed that, instead of

revealing anything independent of the judging process, judgment is just the adjustment and no more. It is a reorganization of experience, an attempt at control. All this looks to me like a misstatement of the facts. Judgment *claims* to be no such thing. It does not function as such a thing. When I make any judgment, even the simplest, I may make it as the result of tension, because of a demand for reorganization, in order to secure control of experience; but the judgment *means* for me something quite different. It means decidedly and unequivocally that in reality, apart from the judging process, things exist and operate just as the judgment declares. If it is claimed that this meaning is illusory, I eagerly desire to know on what solid ground its illusoriness can be established. When the conclusion was reached that gravitation varies directly as the mass and inversely as the square of the distance, it was doubtless reached in an evolutionary and pragmatic way; but it claimed to disclose a fact which prevailed before the conclusion was reached, and in spite of the conclusion. Knowledge has been born of the travail of living, but it has been born as knowledge.

When the knowledge character of judgment is insisted on, it seems almost incredible that any one would think of denying or overlooking it. Indeed, current discussions are far from clear on the subject. Pragmatists are constantly denying that they hold the conclusions that their critics almost unanimously draw. There is, therefore, a good deal of confusion of thought yet to be dispelled. Yet there seems to be current a pronounced determination to banish the epistemological problem from logic. This is, to my mind, suspicious, even when epistemology is defined in a way which most epistemologists would not approve. It is suspicious just because we must always ask eventually that most epistemological and metaphysical question: "Is

knowledge true?" To answer, it is true when it functions in a way to satisfy the needs which generated its activity, is, no doubt, correct, but it is by no means adequate. The same answer can be made to the inquiry after the efficiency of any vital process whatever, and is, therefore, not distinctive. We have still to inquire into the specific character of the needs which originate judgments and of the consequent satisfaction. Just here is where the uniqueness of the logical problem is disclosed. With conscious beings, the success of the things they do has become increasingly dependent on their ability to discover what takes place in independence of the knowing process. That is the need which generates judgment. The satisfaction is, of course, the attainment of the discovery. Now to make the judgment itself and not the consequent action the instrumental factor seems to me to misstate the facts of the case. Nothing is clearer than that there is no necessity for knowledge to issue in adjustment. And it is clear to me that increased control of experience, while resulting from knowledge, does not give to it its character. Omniscience could idly view the transformations of reality and yet remain omniscient. Knowledge works, but it is not, therefore, knowledge.

These considerations have peculiar force when applied to that branch of knowledge which is knowledge itself. Is the biological account of knowledge correct? That question we must evidently ask, especially when we are urged to accept the account. Can we, to put the question in its most general form, accept as an adequate account of the logical process a theory which is bound up with some other specific department of human knowledge? It seems to me that we cannot. Here we must be epistemologists and metaphysicians, or give up the problem entirely. This by no means involves the attempt to conceive pure thought set over against pure reality—the kind of epistemology and

metaphysics justly ridiculed by the pragmatist—for knowledge, as already stated, is given to us in concrete instances. How knowledge in general is possible is, therefore, as useless and meaningless a question as how reality in general is possible. The knowledge is given as a fact of life, and what we have to determine is not its non-logical antecedents or its practical consequences, but its constitution as knowledge and its validity. It may be admitted that the question of validity is settled pragmatically No knowledge is true unless it yields results which can be verified, unless it *can* issue in increased control of experience. But I insist again that that fact is not sufficient for an account of what knowledge claims to be. It claims to issue in control because it is true in independence of the control. And it is just this assurance that is needed to distinguish knowledge from what is not knowledge. It is the necessity of exhibiting this assurance which makes it impossible to subordinate logical problems, and forces us at last to questions of epistemology and metaphysics.

As I am interested here primarily in determining the field of logic, it is somewhat outside my province to consider the details of logical theory. Yet the point just raised is of so much importance in connection with the main question that I venture the following general considerations. This is, perhaps, the more necessary because the pragmatic doctrine finds in the concession made regarding the test of validity one of its strongest defenses.

Of course a judgment is not true simply because it is a judgment. It may be false. The only way to settle its validity is to discover whether experience actually provides what the judgment promises, that is, whether the conclusions drawn from it really enable us to control experience. No mere speculation will yield the desired result, no matter with how much formal validity the conclusions may be

drawn. That merely formal validity is not the essential thing, I have pointed out in discussing the relation of logic to mathematics. The test of truth is pragmatic. It is apparent, therefore, that the formal validity does not determine the actual validity. What is this but the statement that the process of judgment is not itself the determining factor in its real validity? It is, in short, only valid judgments that can really give us control of experience. The implications taken up in the judgment must, therefore, be real implications which, as such, have nothing to do with the judging process, and which, most certainly, are not brought about by it. And what is this but the claim that judgment as such is never instrumental? In other words, a judgment which effected its own content would only by the merest accident function as valid knowledge. We have valid knowledge, then, only when the implications of the judgment are found to be independent of the judging process. We have knowledge only at the risk of error. The pragmatic test of validity, instead of proving the instrumental character of judgment, would thus appear to prove just the reverse.

Valid knowledge has, therefore, for its content a system of real, not judged or hypothetical implications. The central problem of logic which results from this fact is not how a knowledge of real implications is then possible, but what are the ascertainable types of real implications. But, it may be urged, we need some criterion to determine what a real implication is. I venture to reply that we need none, if by such is meant anything else than the facts with which we are dealing. I need no other criterion than the circle to determine whether its diameters are really equal. And, in general, I need no other criterion than the facts dealt with to determine whether they really imply what I judge them to imply. Logic appears to me to be really as simple

as this. Yet there can be profound problems involved in the working out of this simple procedure. There is the problem already stated of the most general types of real implication, or, in other words, the time-honored doctrine of categories. Whether there are categories or basal types of existence seems to me to be ascertainable. When ascertained, it is also possible to discover the types of inference or implication which they afford. This is by no means the whole of logic, but it appears to me to be its central problem.

These considerations will, I hope, throw light on the statement that while knowledge works, it is not therefore knowledge. It works because its content existed before its discovery by the knowledge process, and because its content was not effected or brought about by that process. Judgment was the instrument of its discovery, not the instrument which fashioned it. While, therefore, willing to admit that logical processes are vital processes, I am not willing to admit that the problem of logic is radically changed thereby in its formulation or solution, for the vital processes in question have the unique character of knowledge, the content of which is what it claims to be, a system of real implications which existed prior to its discovery.

In the psychological and biological tendencies in logic, there is, however, I think, a distinct gain for logical theory. The insistence that logical processes are both mental and vital has done much to take them out of the transcendental aloofness from reality in which they have often been placed, especially since Kant. So long as thought and object were so separated that they could never be brought together, and so long as logical processes were conceived wholly in terms of ideas set over against objects, there was no hope of escape from the realm of pure hypothesis and conjecture. Locke's axiom that "the mind, in all its thoughts and

reasonings, hath no other immediate object but its own ideas," an axiom which Kant did so much to sanctify, and which has been the basal principle of the greater part of modern logic and metaphysics, is most certainly subversive of logical theory. The transition from ideas to anything else is rendered impossible by it. Now it is just this axiom which the biological tendencies in logic have done so much to destroy. They have insisted, with the greatest right, that logical processes are not set over against their content as idea against object, as appearance against reality, but are processes of reality itself. Just as reality can and does function in a physical or a physiological way, so also it functions in a logical way. The state we call knowledge becomes, thus, as much a part of the system of things as the state we call chemical combination. The problem how thought can know anything becomes, therefore, as irrelevant as the problem how elements can combine at all. The recognition of this is a great gain, and the promise of it most fruitful for both logic and metaphysics.

But, as I have tried to point out, all this surrendering of pure thought as opposed to pure reality, does not at all necessitate our regarding judgment as a process which makes reality different from what it was before. Of course there is one difference, namely, the logical one; for reality prior to logical processes is unknown. As a result of these processes it becomes known. These processes are, therefore, responsible for a known as distinct from an unknown reality. But what is the transformation which reality undergoes in becoming known? When it becomes known that water seeks its own level, what change has taken place in the water? It would appear that we must answer, none. The water which seeks its own level has not been transformed into ideas or even into a human experience. It appears to remain, as water, precisely what it was before.

The transformation which takes place, takes place in the one who knows, a transformation from ignorance to knowledge. Psychology and biology can afford us the natural history of this transformation, but they cannot inform us in the least as to why it should have its specific character. That is given and not deduced. The attempts to deduce it have, without exception, been futile. That is why we are forced to take it as ultimate in the same way we take as ultimate the specific character of any definite transformation. To my mind, there is needed a fuller and more cordial recognition of this fact. The conditions under which we, as individuals, know are certainly discoverable, just as much as the conditions under which we breathe or digest. And what happens to things when we know them is also as discoverable as what happens to them when we breathe them or digest them.

But here the idealist may interpose that we can never know what happens to things when we know them, because we can never know them before they become known. I suppose I ought to wrestle with this objection. It is an obvious one, but, to my mind, it is without force. The objection, if pursued, can carry us only in a circle. The problem of knowledge is still on our hands, and every logician of whatever school, the offerer of this objection also, has, nevertheless, attempted to show what the transformation is that thought works, for all admit that it works some. Are we, therefore, engaged in a hopeless task? Or have we failed to grasp the significance of our problem? I think the latter. We fail to recognize that, in one way or other, we do solve the problem, and that our attempts to solve it show quite clearly that the objection under consideration is without force. Take, for instance, any concrete case of knowledge, the water seeking its own level, again. Follow the process of knowledge to the full-

est extent, we never find a single problem which is not solvable by reference to the concrete things with which we are dealing, nor a single solution which is not forced upon us by these things rather than by the fact that we deal with them. The transformation wrought is thus discovered, in the progress of knowledge itself, to be wrought solely in the inquiring individual, and wrought by repeated contact with the things with which he deals. In other words, all knowledge discloses the fact that its content is not created by itself, but by the things with which it is concerned.

It is quite possible, therefore, that knowledge should be what we call transcendent and yet not involve us in a transcendental logic. That we should be able to know without altering the things we know is no more and no less remarkable and mysterious than that we should be able to digest by altering the things we digest. In other words, the fact that digestion alters the things is no reason that knowledge should alter them, even if we admit that logical processes are vital and subject to evolution. Indeed, if evolution teaches us anything on this point, it is that knowledge processes are real just as they exist, as real as growth and digestion, and must have their character described in accordance with what they are. The recognition that knowledge can be transcendent and yet its processes vital seems to throw light on the difficulty evolution has encountered in accounting for consciousness and knowledge. All the reactions of the individual seem to be expressible in terms of chemistry and physics without calling in consciousness as an operating factor. What is this but the recognition of its transcendence, especially when the conditions of conscious activity are quite likely expressible in chemical and physical terms? While, therefore, biological considerations result in the great gain of giving concrete reality to the processes of knowledge, the gain is

lost, if knowledge itself is denied the transcendence which it so evidently discloses.

IV

The argument advanced in this discussion has had the aim of emphasizing the fact that in knowledge we have actually given, as content, reality as it is in independence of the act of knowing, that the real world is self-existent, independent of the judgments we make about it. This fact has been emphasized in order to confine the field of logic to the field of knowledge as thus understood. In the course of the argument, I have occasionally indicated what some of the resulting problems of logic are. These I wish now to state in a somewhat more systematic way.

The basal problem of logic becomes, undoubtedly, the metaphysics of knowledge, the determination of the nature of knowledge and its relation to reality. It is quite evident that this is just the problem which the current tendencies criticised have sought, not to solve, but to avoid or set aside. Their motives for so doing have been mainly the difficulties which have arisen from the Kantian philosophy in its development into transcendentalism, and the desire to extend the category of evolution to embrace the whole of reality, knowledge included. I confess to feeling the force of these motives as strongly as any advocate of the criticised opinions. But I do not see my way clear to satisfying them by denying or explaining away the evident character of knowledge itself. It appears far better to admit that a metaphysics of knowledge is as yet hopeless, rather than so to transform knowledge as to get rid of the problem; for we must ultimately ask after the truth of the transformation. But I am far from believing that a metaphysics of knowledge is hopeless. The biological tendencies themselves seem to furnish us with much material for

at least the beginning of one. Reality known is to be set over against reality unknown or independent of knowledge, not as image to original, idea to thing, phenomena to noumena, appearance to reality; but reality as known is a new stage in the development of reality itself. It is not an external mind which knows reality by means of its own ideas, but reality itself becomes known through its own expanding and readjusting processes. So far I am in entire agreement with the tendencies I have criticised. But what change is effected by this expansion and readjustment? I can find no other answer than this simple one: the change to knowledge. And by this I mean to assert unequivocally that the addition of knowledge to a reality hitherto without it is simply an addition to it and not a transformation of it. Such a view may appear to make knowledge a wholly useless addition, but I see no inherent necessity in such a conclusion. Nor do I see any inherent necessity of supposing that knowledge must be a useful addition. Yet I would not be so foolish as to deny the usefulness of knowledge. We have, of course, the most palpable evidences of its use. As we examine them, I think we find, without exception, that knowledge is useful just in proportion as we find that reality is not transformed by being known. If it really were transformed in that process, could anything else than confusion result from the multitude of knowing individuals?

To me, therefore, the metaphysics of the situation resolves itself into the realistic position that a developing reality develops, under ascertainable conditions, into a known reality without undergoing any other transformation, and that this new stage marks an advance in the efficiency of reality in its adaptations. My confidence steadily grows that this whole process can be scientifically worked out. It is impossible here to justify my confidence in detail, and I must leave the matter with the following sug-

gestion. The point from which knowledge starts and to which it ultimately returns is always some portion of reality where there is consciousness, the things, namely, which, we are wont to say, are in consciousness. These things are not ideas representing other things outside of consciousness, but real things, which, by being in consciousness, have the capacity of representing *each other,* of standing for or implying each other. Knowledge is not the creation of these implications, but their successful systematization. It will be found, I think, that this general statement is true of every concrete case of knowledge which we possess. Its detailed working out would be a metaphysics of knowledge, an epistemology.

Since knowledge is the successful systematization of the implications which are disclosed in things by virtue of consciousness, a second logical problem of fundamental importance is the determination of the most general types of implication with the categories which underlie them. The execution of this problem would naturally involve, as subsidiary, the greater part of formal and symbolic logic. Indeed, vital doctrines of the syllogism, of definition, of formal inference, of the calculus of classes and propositions, of the logic of relations, appear to be bound up ultimately with a doctrine of categories; for it is only a recognition of basal types of existence with their implications that can save these doctrines from mere formalism. These types of existence or categories are not to be regarded as free creations or as the contributions of the mind to experience. There is no deduction of them possible. They must be discovered in the actual progress of knowledge itself, and I see no reason to suppose that their number is necessarily fixed, or that we should necessarily be in possession of all of them. It is requisite, however, that in every case categories should be incapable of reduction to each other.

A doctrine of categories seems to me to be of the greatest importance in the systematization of knowledge, for no problem of relation is even stateable correctly before the type of existence to which its terms belong has been first determined. I submit one illustration to reinforce this general statement, namely, the relation of mind to body. If mind and body belong to the same type of existence, we have one set of problems on our hands; but if they do not, we have an entirely different set. Yet volumes of discussion written on this subject have abounded in confusion, simply because they have regarded mind and body as belonging to radically different types of existence and yet related in terms of the type to which one of them belongs. The doctrine of parallelism is, perhaps, the epitome of this confusion.

The doctrine of categories will involve not only the greater part of formal and symbolic logic, but will undoubtedly carry the logician into the doctrine of method. Here it is to be hoped that recent tendencies will result in effectively breaking down the artificial distinctions which have prevailed between deduction and induction. Differences in method do not result from differences in points of departure, or between the universal and the particular, but from the categories, again, which give the method direction and aim, and result in different types of synthesis. In this direction, the logician may hope for an approximately correct classification of the various departments of knowledge. Such a classification is, perhaps, the ideal of logical theory.

THE RELATIONS OF ETHICS

BY WILLIAM RITCHIE SORLEY

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THERE are many departments of inquiry whose scope is so well defined by the consensus of experts that one may proceed, almost without preliminary, to mark off the boundaries of one science from other departments, to investigate the relations in which it stands to them, and to exhibit the place which each occupies in the whole scheme of human knowledge. In other departments opinion differs not only regarding special problems and results, but concerning the whole nature of the science and its relation to connected subjects. The study of ethics still belongs to this latter group. In it there is no consensus of experts. Competent scholars hold diametrically opposed views as to its scope. They differ not merely in the answers they give to ethical questions, but in their views as to what the fundamental question of ethics is. And this opposition of opinion as to its nature is connected with a difference of view regarding the relation of ethics to the sciences. By many investigators it is set in line with the sciences of biology, psychology, and sociology; and its problems are formulated and discussed by the application of the same

historical method as those sciences employ. On the other hand, it is maintained that ethics implies and requires a concept so different from the concepts used by the historical and natural sciences as to give its problem an altogether distinct character and to indicate for it a far more significant position in the whole scheme of human thought.

The question of the relation of ethics to the sciences implies a view of the nature of ethics itself and, in particular, of the fundamental concept used in ethical judgments. If the nature of this concept and its relation to the concepts employed in other branches of inquiry can be determined, the relations of ethics will become clear of themselves. The problem of this paper will receive its most adequate solution—so far as the time at my disposal permits—by an independent inquiry into the nature of the ethical concept in relation to the concepts used in other sciences.

The immediate judgments of experience fall into two broadly contrasted classes, which may be described in brief as judgments of fact and judgments of worth. The former are the foundations on which the whole edifice of science (as the term is commonly used) is built. Science has no other object than to understand the relations of facts as exhibited in historical sequence, in causal interconnection, or in the logical interdependence which may be discovered amongst their various aspects. In its beginnings it may have arisen as an aid to the attainment of practical purposes: it is still everywhere yoked to the chariot of man's desires and aims. But it has for long vindicated an independent position for itself. It may be turned to what uses you will; but its essential spirit stands aloof from these uses. It has one interest only—to know what happens and how. Otherwise it is indifferent to all purposes alike. It studies with equal mind the slow growth of a plant or the swift destruction wrought by the torpedo, the reign

of a Caligula or of a Victoria; it takes no side, but observes and describes all "just as if the question were of lines, planes, and solids." Mathematical method does not limit its range, but it typifies its attitude of indifference to every interest save one,—that of knowing the what and how of things.

We can conceive an intelligence of this nature, a pure intelligence, or mere intelligence, to whose understanding all the relations of things are evident, with the prophetic power of the Laplacian Demon and the gift of tongues to make its knowledge clear, and yet unable to distinguish between good and evil or to see beauty or ugliness in nature. We can conceive such an intelligence; but it is an unreality, a mere abstraction from the scientific aspect of human intelligence. Pure intelligence of this sort does not exist in man, and we have no grounds for asserting its existence anywhere. In the experience which forms the basis of mental life, judgments of reality are everywhere combined with and colored by judgments of worth. And the latter are as insistent as the former, and make up as large a part of our experience. If we go back to the original judgments of experience, we find that they are not only of the form "it is here or there," "it is of this nature or that," "it has such and such effects;" just as a large part of our experience is of another order which may be expressed in judgments of the form "it is good or evil," "it is fair or foul."

Nor does the way in which scientific judgments are elaborated give any rationale of the distinction between good and evil. If we ask of science "What is good?" it can give no relevant answer to the question. Strictly speaking, it does not understand the meaning of the question at all. The ball has gone out of bounds; and science cannot touch it until it has been thrown back into the field. It can say what is, and what will happen, and it can describe

the methods or laws by which things come to pass; that is all; it has only one law for the just and the unjust.

But science is very resourceful, and is able to deal with judgments of worth from its own point of view. For these judgments also are facts of individual experience: they are formed by human minds under certain conditions, betray certain relations to the judgments of fact with which they are associated, and are connected with an environment of social institutions and physical conditions of life: they have a history therefore. And in these respects they become part of the material for science: and a description of them can be given by psychological and historical methods.

The general nature and results of the application of these methods to ethics are too well known to need further comment, too well established to require defense. But these results may be exaggerated and have been exaggerated. When all has been said and done that the historical method can say and do, the question "What is good?" is found to remain exactly where it was. We may have learned much as to the way in which certain kinds of conduct in certain circumstances promote certain ends, and as to the gradual changes which men's ideas about good and evil, virtue and vice, have passed through; but we have not touched the fundamental question which ethics has to face —the question of the nature of worth or goodness or duty.

And yet it is this question only which gives significance to the problems on which historical evolution has been able to throw light. Moral ideas and moral institutions have all along been effective factors in human development, as well as the subject of development themselves. And the secret of their power has lain in this that men have believed in those ideas as expressing a moral imperative or a moral end, and that they have looked upon moral institu-

tions as embodiments of something which has worth for man or a moral claim upon his devotion. These ideas and institutions would have had no power apart from this belief in their validity.

But was this belief true? Were the ideas or institutions valid? This question the man of science, as sociologist or historian, does not answer and has no means of answering. He can show their adaptation or want of adaptation to certain ends, but he can say nothing about the validity of these ends themselves. It is implied in their efficiency that these ends were conceived as having moral value or moral authority. But to what ends does this moral value or authority truly belong? and what is its significance?—these are questions which the positive science (such as psychology and sociology) cannot touch and which must be answered by other methods than those which they employ.

The moral concept is expressed in various ways and by a variety of terms,—right, duty, merit, virtue, goodness, worth. And these different terms indicate different aspects opened up by a single new point of view. Thus "right" seems to imply correspondence with a standard or rule, which standard or rule is some moral law or ideal of goodness; and "merit" indicates performance of the right, perhaps in victory over some conflicting desire; and "virtue" means a trait of character in which performance of this sort has become habitual. The term "worth" has conveniences which have led to its having considerable vogue in ethical treatises since the time of Herbart; it lends itself easily to psychological manipulation; but it does not seem to refer to a concept fundamentally distinct from goodness. But between "goodness" and "duty" there seems to be this difference at any rate, that the latter term refers definitely to something to be done by a voluntary agent, whereas, in calling something "good," we may have no thought of action at all, but only see and name a quality.

There lies here therefore a difference which is not a mere difference of expression.

On the one hand it may be held that good is a quality which belongs to certain things and has no special and immediate reference to volition: that we say this or that is good as we say that something else is heavy or green or positively electrified. No relation to human life at all may be implied in the one form of judgment any more than in the other. That relation will only follow by way of application to circumstances. Just as a piece of lead may serve as a letter-weight because it is heavy, so certain actions may come to be our duty becauses they lead to the realization of something which is objectively good in quality.

According to the other view goodness has reference in its primary meaning to free self-conscious agency. The good is that which ought to be brought into existence: goodness is a quality of things, but only in a derivative regard because these things are produced by a good will. It is objective, too, inasmuch as it unites the individual will with a law or ideal which has a claim upon the will; but it does not in its primary meaning indicate something out of relation to the will: if there were no will there would be no law; apart from conscious agency good and evil would disappear.

The question thus raised is one of real and fundamental importance. "Ethics" by its very name may seem to have primary reference to conduct; and that is the view which most moralists have, in one way or another, adopted. But the other view which gives to the concept "good" an independence of all relation to volition is not always definitely excluded, even by these moralists; by others it has been definitely maintained: it seems implied in Plato's idealism, at one stage of its development; and quite recently a doc-

trine of the principles of ethics has been worked out which is based on its explicit recognition.[1]

If we would attempt to decide between these two conflicting views of the ethical concept, we must, in the first place, imitate the procedure of science and examine the facts on which the concept is based. To get to the meaning of such scientific concepts as "mass," "energy," or the like, we begin by a consideration of the facts which the concepts are introduced to describe. These facts are in the last resort the objects of sense perception. No examination of these sense percepts will, as we have seen, yield the content of the ethical concept; good and evil are not given in sense perception—they are themselves an estimate of, or way of regarding, the immediate material of experience. Moral experience is thus in a manner reflex, as so many of the English moralists have called it. Its attitude to things is not merely receptive; and the concepts to which it gives rise have not mere understanding in view. Objects are perceived as they occur; and experience of them is the groundwork of science. There is also, at the same time, an attitude of approbation or disapprobation; this attitude is the special characteristic of moral experience; and from moral experience the ethical concept is formed.

This reflex experience, or reflex attitude to experience, is exhibited in different ways. There is, to begin with, the appreciation of beauty in its various kinds and degrees and the corresponding depreciation of ugliness or deformity. These give rise to the concepts and judgments of æsthetics. They are closely related to moral approbation and disapprobation, so closely that there has always been a tendency amongst a school of moralists to strain the facts by identifying them. A certain looseness in our use of terms favors this tendency. For we do often use good of a work of art

[1] *Principia Ethica,* by G. E. Moore (1903).

or even scene in nature when we mean beautiful. But if we reflect on and compare our mental attitudes in commending, say, a sunset and self-sacrifice, it seems to me that there can be no doubt that the two attitudes are different. Both objects may be admired; but both are not, in the same sense, approved. It is hard to express this difference otherwise than by saying that the moral attitude is present in the one and absent in the other. But the difference is brought out by the fact that our æsthetical and moral attitudes towards the same experience may diverge from one another. We may admire the beauty of that which we condemn as immoral. De Quincey saw a fine art in certain cases of murder; the finish and perfection of wickedness may often stir a certain artistic admiration, especially if we lull the moral sense to sleep. And, on the other hand, moral approval is often tempered by a certain æsthetic depreciation of those noble characters who do good awkwardly, without the ease and grace of a gentleman. John Knox and Mary Queen of Scots (if I may assume for the moment an historical judgment which may need qualification) will each have his or her admirers according as the moral or æsthetic attitude preponderates—the harsh tones of the one appealing to the law of truth and goodness, the other an embodiment of the beauty and gaiety of life, "without a moral sense, however feeble."

Nor is æsthetic appreciation the only other reflex attitude which has a place in our experience side by side with the moral. Judgments about matters of fact and relations of ideas are discriminated as true or false; an ideal of truth is formed; and conditions of its realization are laid down. Here again we have a concept and class of judgments analogous to our æsthetical and ethical concepts and judgments, but not the same as them, and not likely to be confused with them.

Beside these may be put a whole class of judgments of worth which may be described as judgments of utility. We estimate and approve or disapprove various facts of experience according to their tendency to promote or interfere with certain ends or objects of desire. That moral judgments are to be identified with a special class of these judgments of utility is a thesis too well known to require discussion here, and too important to admit of discussion in a few words. But it may be pointed out that it is only in a very special and restricted sense of the term "utility" that judgments of utility have ever been identified with moral judgments. The "jimmy" is useful to the burglar, as his instruments are useful to the surgeon; and they are in both cases appreciated by the same kind of reflective judgment. Judgments of utility are all of them, properly speaking, judgments about means to ends; and the ends may and do differ; while it is only by a forced interpretation that all these ends are sometimes and somehow made to resolve themselves into pleasure.

It is enough, however, for my present purpose to recognize the *prima facie* distinction of moral judgments or judgments of goodness from other judgments of worth, such as those of utility, of beauty, and of truth (in the sense in which these last also are judgments of worth). Had the question of the origin and history of the moral judgment been before us, a great deal more might have been necessary. For our present purpose what has been already said may be sufficient: it was required in order to enable us to approach the consideration of the question already raised concerning the application and meaning of the moral concept.

The question is, Does our moral experience support the assignment of the predicate "good" or "bad" to things regarded as quite independent of volition or consciousness?

At first sight it may seem easy to answer the question in the affirmative. We do talk of sunshine and gentle rain and fertile land as good, and of tornadoes and disease and death as bad. But I think that when we do so, in nine cases out of ten, our "good" or "bad" is not a moral good or bad; they are predicates of utility or sometimes æsthetic predicates, not moral predicates; and we recognize this in recognizing their relativity: the fertile land is called good because its fertility makes it useful to man's primary needs; but the barren and rocky mountain may be better in the eyes of the tourist, though the farmer would call it bad land. There is an appreciation, a judgment of worth in the most general sense, in such experiences; but they are in most cases without the special feature of moral approbation or disapprobation.

There remains, however, the tenth case in which the moral predicate does seem to be applied to the unconscious. One may instance J. S. Mill's passionate impeachment of the course of nature, in which "habitual injustice" and "nearly all the things which men are hanged or imprisoned for doing to one another" are spoken of as "nature's every-day performances;"[1] and a similar indictment was brought by Professor Huxley, twenty years after the publication of Mill's essay, against the cosmic process for its encouragement of selfishness and ferocity.[2] These are only examples. Literature is full of similar reflections on the indiscriminate slaughter wrought by the earthquake or the hurricane, and on the sight of the wicked flourishing or of the righteous begging his bread; and these reflections find an echo in the experience of most men.

But the nature of this experience calls for remark.

In the first place, if we look more closely at the argu-

[1] J. S. Mill, *Three Essays on Religion*, pp. 35, 38.

[2] T. H. Huxley, *Evolution and Ethics* (Romanes Lecture).

ments of Mill or Huxley, we see that both are cases of criticism of a philosophical theory. Mill was refuting a view which he held (and rightly held) to have influence still on popular thought, though it might have ceased to be a living ethical theory—the doctrine that the standard of right and wrong was to be found in nature; it was in keeping with his purpose, therefore, to speak of the operations of nature as if they were properly the subject of moral praise or blame. In the same way, when Huxley wrote, the old doctrine which Mill regarded as philosophically extinct and only surviving as a popular error had been revived by the impetus which the theory of evolution had given to every branch of study; and Huxley was criticising the evolutionist ethics of Spencer and others who looked for moral guidance to the course of evolution. He, therefore, was led to speak of the cosmic process as a possible subject of moral predicates, not necessarily because he thought that application appropriate, but in order to demonstrate the hollowness of the ethics of evolution by showing that if the moral predicate could be applied at all, then the appropriate adjective would be not "good" but "bad."

Perhaps there is more than this in Huxley; and Mill's expressions often betray a direct and genuine moral condemnation of the methods of nature as methods of wickedness; and, still more clearly, this immediate moral disapproval may be found in expressions of common experience as yet uncolored by philosophy. But if we examine these we find that, while there is no reference to philosophical theories about nature, the things approved or condemned are yet looked upon as implying consciousness. In the lower stages of development this implication is simply animistic; at a later period it becomes theological. But throughout experience moral judgments upon nature are not passed upon mere nature. Its forces are regarded as

expressing a purpose or mind; and it is this that is condemned or approved. The primitive man and the child do not merely condemn the misdoings of inanimate objects; they wreak their vengeance upon them or punish them: and this is a consequence of their animistic interpretation of natural forces. Gradually, in the mental growth of the child, this animistic interpretation of things gives place to an understanding of the natural laws of their working; and at the same time and by the same degrees, the child ceases to inflict punishment upon the chair that has fallen on him or to condemn its misdemeanor. Here the moral judgment is displaced by the causal judgment; and the reason of its displacement is the disappearance of mind or purpose from amongst the phenomena. When the child comes to understand that the chair falls by "laws of nature" which are not the expression of will, like the acts done by himself or his companions, he ceases to disapprove or to resent, though he does not cease to feel pain or to improve the circumstances by setting the chair firmly on the floor. The recognition of natural causation as all that there is in the case leaves no room for the moral attitude. So true is this that the same result is sometimes thought to be a consequence of the scientific understanding even of what is called moral causation, "tout comprendre c'est tout pardonner"—as if knowledge of motive and circumstances were sufficient to dispense with praise or blame.

Moral judgments of a more mature kind on the constitution and course of nature form the material for optimistic and pessimistic views of the world—at least, when these views rise above the assertion of a preponderance of pleasure or of pain in life. But, so far as I can see, in such moral judgments nature is never looked upon as consisting of dead mechanical sequences. It is because it is looked upon as the expression of a living will or as in some way—

perhaps very vaguely conceived—animated by purpose or consciousness, that we regard it as morally good or evil. Apart from some such theological conception, it does not seem to me that the nature of things calls out the attitude of moral approval or disapproval. Things are estimated as useful for this or that end, they are seen and appreciated as beautiful or the reverse, without any reference to them as due to an inspiring or originating mind; and in one or other of these references the terms "good" or "bad" may be used. But when we use the term good in its specifically moral signification, we do not apply it to the inanimate, except in a derivative way, on account of the relation in which these inanimate things stand to the moral ends and character of conscious beings.

So far, therefore, as the evidence of moral experience goes, it does not support the view that the "good" is a quality which belongs to things out of relation to self-conscious activity. And, in so far, the peculiarity of the moral experience would seem to be better brought out by the conception "ought" than by the conception "good."

But here a difficulty arises at once. For how can we say that anything ought to be done or to be except on the assumption that it is antecedently good? Is not such antecedent and independent goodness necessary in order to justify the assertion that any one ought to produce it?

The question undoubtedly points to a difficulty; and if that difficulty can be solved it may help to bring out the true significance of the moral concept. The judgment which assigns the duty of an individual—according to which I or any one ought to adopt a certain course of action—involves a special application of the moral concept. It binds the individual to a certain objective rule or end. The individual's desires as mere facts of experience may point in an altogether different direction; the purpose or volition

contemplated and approved by the moral judgment has in view the union of individual striving with an end which is objective and, as objective, universal. This union involves an adaptation of two things which may fall asunder, and in every case of evil volition do fall asunder. And the adaptation may be regarded from either side; on the side of the individual, application to his individuality is implied; the duty of one man is not just the same as the duty of any other; he has his own special place and calling. But he is connected with a larger purpose which in his consciousness becomes both an ideal and a law, while its application is not limited to his individuality or his circumstances.

All this is implied in the moral judgment. It is not limited to one individual consciousness or volition. But it does not follow that the predicate "good," in the ethical meaning of the term, is or can be applied out of relation to consciousness altogether. At the earliest stages of moral development we find it applied unhesitatingly wherever conscious activity is supposed to be present—to anything that is regarded as the embodiment of spirit; and it is applied to the universe as a whole when the universe is thought of as the product of mind. "Good" is not even limited to an actual existent; it neither implies nor denies actual existence. "Such and such, if it existed, would be a good" is as legitimate though not so primitive an expression of the moral judgment as "this existent is good." But it does imply a relation to existence. It does not even seem possible to distinguish except verbally between "good" and "ought to be." And this "ought" seems to imply a reference to a purpose through which the idea is to be realized.

This conception "ought to be" is not the same as the concept "ought to be done by me." The latter is an application of the more general concept to a special individual in special circumstances; and this is the common meaning

of the concept duty. The former is the more general concept of "goodness." It may be called objective, because it does not refer to any individual state of mind; it is universal because independent of the judgments and desires of the individual; and when the goodness is not due to its tendency towards some further end, it may also be called absolute.

The point of the whole argument can thus be made clear if we bear in mind the familiar distinction between "good in itself" and "good for me now." That the latter has always a relation to consciousness is obvious: it is something to be done or experienced by me. But there must be some ground why anything is to be or ought to be done or experienced by me at any time. Present individual activity must rest upon or be connected with some wider or objective basis. What is good for me points to and depends upon something which is not merely relatively good, but good in itself or absolutely. Yet it does not follow that this good in itself is necessarily absolute in the sense of having significance apart altogether from consciousness. Its absoluteness consists in independence of individual consciousness or feeling, not in independence of consciousness altogether. It is objective rather than absolute in the literal sense of the term. The good in itself, like the relative good, is one aspect which can only belong to a consciousness—to purpose. The moral judgment on things—either on the universe as a whole, or on anything in the universe which is not regarded as due to the will of man—is only justified if we regard these things as in some way expressing consciousness; either as directly due to it, or as aiding it, or as in conflict with it. From any other point of view, to speak of things as good or evil (unless in some non-ethical sense of these terms) seems out of place, and is unsupported by the mode of application which belongs to the immediate judgments of the

moral consciousness. If the moral concept has significance beyond the range of the feelings and desires of men, it is because the objects to which it applies are the expression of mind.

This is not put forward as a vindication of a spiritual idealism. It is only a small contribution towards the meaning of "good." A comprehensive idealism may not be the only view of reality with which the conclusions reached so far will harmonize. But it is the view with which they harmonize most simply. The conception of a purpose to which all the events of the world are related is a form in which the essential feature of idealism may be expressed; the view of this purpose as good makes the idealism at the same time a moral interpretation of reality, and allows of our classing each distinguishable event as good or evil according as it tends to the furtherance or hindrance of that purpose.

This doctrine of the significance and application of the ethical concept would enable us to reach a definite view of the nature of ethics and of the way in which it is related to the sciences and to metaphysics. The ethical concept is based upon the primary facts of the moral consciousness, just as scientific concepts have as their basis the facts of direct experience. The primary facts of the moral consciousness are themselves of the nature of judgment—they are approbations or disapprobations. But all facts of experience involve judgments, though these judgments may be only of the form "it is here" or "it is of this or that nature." Again, the primary ethical facts or judgments cannot be assumed to be of unquestionable validity: we may approve what is not worthy of approval, or disapprove what ought to have been approved. Our moral judgments claim validity; and their claim is of the nature of an assertion, not that one simply feels in such and such a way,

but that something ought or ought not to be. They imply an objective standard. But the objective standard, when more clearly understood, may modify or even reverse them. Our primary ethical judgments—all our ethical judgments, indeed—stand in need of revision and criticism; and they receive this revision and criticism in the course of the elaboration of the ethical concept and of its application to the worlds of fact and possibility. In the same way it may be contended that the direct judgments of experience upon which science is based need criticism and correction; though their variation may be less in amount than the variation of moral judgments. The color-blind man identifies red with green, and his judgment on this point has to be reversed; the hypersensitive subject often confuses imagines with percepts; exact observation needs a highly trained capacity. The correction and criticism which is need come from objective standards; and these are the result of the comparison of many experiences and the work of many minds.

It is no otherwise in the case of ethics. Criticism brings to light inconsistencies in the primary judgments of approbation and disapprobation as well as in the later developments of the moral judgment. And these inconsistencies must be dealt with in a way similar to that in which we deal with inconsistencies in the judgments of perception and of science. The objective standard is not itself given once for all; it has to be formed by accumulation and comparison of moral experiences. Like the experiences on which science is based, these have to be made as far as possible harmonious, and analysis has to be employed to bring out the element of identity which often lurks behind apparent contradiction. They have also to be made as comprehensive as possible, so that they may be capable of application to all relevant facts, and that the scattered de-

tails of the moral consciousness may be welded into an harmonious system. In these general respects the criticism of ethical concepts proceeds upon the same lines as the criticism of scientific concepts. The difference lies in the concepts themselves, for ethics involves a point of view to which science must always remain a stranger.

THE FUNDAMENTAL CONCEPTIONS AND METHODS OF MATHEMATICS

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I. *Old and New Definitions of Mathematics*

I am going to ask you to spend a few minutes with me in considering the question: what is mathematics? In doing this I do not propose to lay down dogmatically a precise definition; but rather, after having pointed out the inadequacy of traditional views, to determine what characteristics are common to the most varied parts of mathematics but are not shared by other sciences, and to show how this opens the way to two or three definitions of mathematics, any one of which is fairly satisfactory. Although this is, after all, merely a discussion of the meaning to be attached to a name, I do not think that it is unfruitful, since its aim is to bring unity into the fundamental conceptions of the science with which we are concerned. If any of you, however, should regard such a discussion of the meaning of words as devoid of any deeper significance, I will ask you to regard this question as merely a bond by means of which I have found it convenient to unite what I have to say on the fundamental conceptions and methods of what, with or without definition, we all of us agree to call mathematics.

The old idea that mathematics is the science of quantity, or that it is the science of space and number, or in-

deed that it can be characterized by any enumeration of several more or less heterogeneous objects of study, has pretty well passed away among those mathematicians who have given any thought to the question of what mathemtaics really is. Such definitions, which might have been intelligently defended at the beginning of the nineteenth century, became obviously inadequate as subjects like projective geometry, the algebra of logic, and the theory of abstract groups were developed; for none of these has any necessary relation to quantity (at least in any ordinary understanding of that word), and the last two have no relation to space. It is true that such examples have had little effect on the more or less orthodox followers of Kant, who regard mathematics as concerned with those conceptions which are obtained by direct intuition of time and space without the aid of empirical observation. This view seems to have been held by such eminent mathematicians as Hamilton and DeMorgan; and it is a very difficult position to refute, resting as it does on a purely metaphysical foundation which regards it as certain that we can evolve out of our inner consciousness the properties of time and space. According to this view the idea of quantity is to be deduced from these intuitions; but one of the facts most vividly brought home to pure mathematicians during the last half-century is the fatal weakness of intuition when taken as the logical source of our knowledge of number and quantity.[1]

The objects of mathematical study, even when we confine our attention to what is ordinarily regarded as *pure* mathematics are, then, of the most varied description; so that, in order to reach a satisfactory conclusion as to what really characterizes mathematics, one of two methods is

[1] I refer here to such facts as that there exists continuous functions without derivatives, whereas the direct untutored intuition of space would lead any one to believe that every continuous curve has tangents.

open to us. On the one hand we may seek some hidden resemblance in the various objects of mathematical investigation, and having found an aspect common to them all we may fix on this as the one true object of mathematical study. Or, on the other hand, we may abandon the attempt to characterize mathematics by means of its *objects of study,* and seek in its *methods* its distinguishing characteristic. Finally, there is the possibility of our combining these two points of view. The first of these methods is that of Kempe, the second will lead us to the definition of Benjamin Peirce, while the third has recently been elaborated at great length by Russell. Other mathematicians have naturally followed out more or less consistently the same ideas, but I shall nevertheless take the liberty of using the names Kempe, Peirce, and Russell as convenient designations for these three points of view. These different methods of approaching the question lead finally to results which, without being identical, still stand in the most intimate relation to one another, as we shall now see. Let us begin with the second method.

II. *Peirce's Definition*

More than a third of a century ago Benjamin Peirce wrote:[1] *Mathematics is the science which draws necessary conclusions.* According to this view there is a mathematical element involved in every inquiry in which exact reasoning is used. Thus, for instance,[2] a jury listening to the attempt of the counsel for the prisoner to prove an alibi in a criminal case might reason as follows: "If the witnesses are telling the truth when they say that the prisoner was in St. Louis at the moment the crime was committed in Chicago,

[1] *Linear Associative Algebra,* Lithographed 1870. Reprinted in the *American Journal of Mathematics,* vol. IV.

[2] This illustration was suggested by the remarks by J. Richard, *Sur la philosophie des mathématiques.* Paris, Gauthier-Villars, 1903, p. 50.

and if it is true that a person cannot be in two places at the same time, it follows that the prisoner was not in Chicago when the crime was committed." This, according to Peirce, is a bit of mathematics; while the further reasoning by which the jury would decide whether or not to believe the witnesses, and the reasoning (if they thought any necessary) by which they would satisfy themselves that a person cannot be in two places at once, would be inductive reasoning, which can give merely a high degree of probability to the conclusion, but never certainty. This mathematical element may be, as the example just given shows, so slight as not to be worth noticing from a practical point of view. This is almost always the case in the transactions of daily life and in the observational sciences. If, however, we turn to such subjects as chemistry and mineralogy, we find the mathematical element of considerable importance, though still subordinate. In physics and astronomy its importance is much greater. Finally in geometry, to mention only one other science, the mathematical element predominates to such an extent that this science has been commonly rated a branch of pure mathematics, whereas, according to Peirce, it is as much a branch of applied mathematics as is, for instance, mathematical physics.

It is clear from what has just been said that, from Peirce's point of view, mathematics does not necessarily concern itself with quantitative relations, and that any subject becomes capable of mathematical treatment as soon as it has secured data from which important consequences can be drawn by exact reasoning. Thus, for example, even though psychologists be right when they assure us that sensations and the other objects with which they have to deal cannot be measured, we need still not necessarily despair of one day seeing a mathematical psychology, just as we already have a mathematical logic.

I have said enough, I think, to show what relation Peirce's conception of mathematics has to the applications. Let us then turn to the definition itself and examine it a little more closely. You have doubtless already noticed that the phrase, "the science which draws necessary conclusions," contains a word which is very much in need of elucidation. What is a *necessary conclusion?* Some of you will perhaps think that the conception here involved is one about which, in a concrete case at least, there can be no practical diversity of opinion among men with well-trained minds; and in fact when I spoke a few minutes ago about the reasoning of the jurymen when listening to the lawyer trying to prove an alibi, I assumed tacitly that this is so. If this really were the case, no further discussion would be necessary, for it is not my purpose to enter into any purely philosophical speculations. But unfortunately we cannot dismiss the matter in this way; for it has happened not infrequently that the most eminent men, including mathematicians, have differed as to whether a given piece of reasoning was exact or not; and, what is worse, modes of reasoning which seem absolutely conclusive to one generation no longer satisfy the next, as is shown by the way in which the greatest mathematicians of the eighteenth century used geometric intuition as a means of drawing what they regarded as necessary conclusions.[1]

I do not wish here to raise the question whether there is such a thing as absolute logical rigor, or whether this whole conception of logical rigor is a purely psychological one

[1] All writers on elementary geometry from Euclid down almost to the close of the nineteenth century use intuition freely, though usually unconsciously, in obtaining results which they are unable to deduce from their axioms. The first few demonstrations of Euclid are criticised from this point of view by Russell in his *Principles of Mathematics*, vol. I, 404-407. Gauss's first proof (1799) that every algebraic equation has a root gives a striking example of the use of intuition in what was intended as an absolutely rigorous proof by one of the greatest and at the same time most critical mathematical minds the world has ever seen.

bound to change with changes in the human mind. I content myself with expressing the belief, which I will try to justify a little more fully in a moment, that as we never have found an immutable standard of logical rigor in the past, so we are not likely to find it in the future. However this may be, so much we can say with tolerable confidence, as past experience shows, that no reasoning which claims to be exact can make any use of intuition, but that it must proceed from definitely and completely stated premises according to certain principles of formal logic. It is right here that modern mathematicians break sharply with the tradition of *a priori* synthetic judgments (that is, conclusions drawn from intuition) which, according to Kant, form an essential part of mathematical reasoning.

If then we agree that "necessary conclusions" must, in the present state of human knowledge, mean conclusions drawn according to certain logical principles from definitely and completely stated premises, we must face the question as to what these principles shall be. Here, fortunately, the mathematical logicians from Boole down to C. S. Peirce, Schröder, and Peano have prepared the field so well that of late years Peano and his followers[1] have been able to make a rather short list of logical conceptions and principles upon which it would seem that all exact reasoning depends.[2] We must remember, however, when we are tempted to put implicit confidence in certain fundamental logical principles, that, owing to their extreme generality and abstractness, no very great weight can be attached to the mere fact that these principles appeal to us as obviously true; for, as I have said, other modes of reasoning which are now universally recognized as faulty have appealed in just this way to the greatest minds of the past. Such confi-

[1] And, independently, Frege.

[2] It is not intended to assert that a single list has been fixed upon. Different writers naturally use different lists.

dence as we feel must, I think, come from the fact that those modes of reasoning which we trust have withstood the test of use in an immense number of cases and in very many fields. This is the severest test to which any theory can be put, and if it does not break down under it we may feel the greatest confidence that, at least in cognate fields, it will prove serviceable. But we can never be sure. The accepted modes of exact reasoning may any day lead to a contradiction which would show that what we regard as universally applicable principles are in reality applicable only under certain restrictions.[1]

To show that the danger which I here point out is not a purely fanciful one, it is sufficient to refer to a very recent example. Independently of one another, Frege and Russell have built up the theory of arithmetic from its logical foundations. Each starts with a definite list of apparently self-evident logical principles, and builds up a seemingly flawless theory. Then Russell discovers that his logical principles when applied to a very general kind of logical *class* lead to an absurdity; and both Frege and Russell have to admit that something is wrong with the foundations which looked so secure. Now there is no doubt that these logical foundations will be somehow recast to meet this difficulty, and that they will then be stronger than ever before.[2] But who shall say that the same thing will not happen again?

It is commonly considered that mathematics owes its certainty to its reliance on the immutable principles of

[1] If the view which I here maintain is correct, it follows that if the term "absolute logical rigor" has a meaning, and if we should some time arrive at this absolute standard, the only indication we should ever have of the fact would be that for a long period, several thousand years let us say, the logical principles in question had stood the test of use. But this state of affairs might equally well mean that during that time the human mind had degenerated, at least with regard to some of its functions. Consider, for instance, the twenty centuries following Euclid when, without doubt, the high tide of exact thinking attained during Euclid's generation had receded.

[2] Cf. Poincaré's view in *La Science et l'Hypothèse*, p. 179, according to which a theory never renders a greater service to science than when it breaks down.

formal logic. This, as we have seen, is only half the truth imperfectly expressed. The other half would be that the principles of formal logic owe such degree of permanence as they have largely to the fact that they have been tempered by long and varied use by mathematicians. "A vicious circle!" you will perhaps say. I should rather describe it as an example of the process known to mathematicians as the method of successive approximations. Let us hope that in this case it is really a convergent process, as it has every appearance of being.

But to return to Peirce's definition. From what are these necesssary conclusions to be drawn? The answer clearly implied is, from any premises sufficiently precise to make it possible to draw necessary conclusions from them. In geometry, for instance, we have a large number of intuitions and fixed beliefs concerning the nature of space: it is homogeneous and isotropic, infinite in extent in every direction, etc.; but none of these ideas, however clearly defined they may at first sight seem to be, gives any hold for exact reasoning. This was clearly perceived by Euclid, who therefore proceeded to lay down a list of axioms and postulates, that is, specific facts which he assumes to be true, and from which it was his object to deduce all geometric propositions. That his success here was not complete is now well known, for he frequently assumes unconsciously further data which he derives from intuition; but his attempt was a monumental one.

III. *The Abstract Nature of Mathematics*

Now a further self-evident point, but one to which attention seems to have been drawn only during the last few years, is this: since we are to make no use of intuition, but only of a certain number of explicitly stated premises,

it is not necessary that we should have any idea what the nature of the objects and relations involved in these premises is.[1] I will try to make this clear by a simple example. In plane geometry we have to consider, among other things, points and straight lines. A point may have a peculiar relation to a straight line which we express by the words, the point lies on the line. Now one of the fundamental facts of plane geometry is that two points determine a line, that is, if two points are given, there exists one and only one line on which both points lie. All the facts that I have just stated correspond to clear intuitions. Let us, however, eliminate our intuition of what is meant by a point, a line, a point lying on a line. A slight change of language will make it easy for us to do this. Instead of points and lines, let us speak of two different kinds of objects, say A-objects and B-objects; and instead of saying that a point lies on a line we will simply say that an A-object bears a certain relation R to a B-object. Then the fact that two points determine a line will be expressed by saying: If any two A-objects are given, there exists one and only one B-object to which they both bear the relation R. This statement, while it does not force on us any specific intuitions, will serve as a basis for mathematical reasoning[2] just as well as the more familiar statement where the terms *points* and *lines* are used. But more than this. Our A-objects, our B-objects, and our relation R may be given an interpretation, if we choose, very different from that we had at first intended.

We may, for instance, regard the A-objects as the straight lines in a plane, the B-objects as the points in the

[1] This was essentially Kempe's point of view in the papers to be referred to presently. In the geometric example which follows it was clearly brought out by H. Wiener: *Jahresbericht d. deutschen Mathematiker-Vereinigung,* vol. I (1891), p. 45.

[2] In conjunction, of course, with further postulates with which we need not here concern ourselves.

same plane (either finite or at infinity), and when an A-object stands in the relation R to a B-object, this may be taken to mean that the line passes through the point. Our statement would then become: Any two lines being given, there exists one and only one point through which they both pass. Or we may regard the A-objects as the men in a certain community, the B-objects as the women, and the relation of an A-object to a B-object as friendship. Then our statement would be: In this community any two men have one, and only one, woman friend in common.

These examples are, I think, sufficient to show what is meant when I say that we are not concerned in mathematics with the nature of the objects and relations involved in our premises, except in so far as their nature is exhibited in the premises themselves. Accordingly mathematicians of a critical turn of mind, during the last few years, have adopted more and more a purely nominalistic attitude towards the objects and relations involved in mathematical investigation. This is, of course, not the crude mixture of nominalism and empiricism of the philosopher Hobbes, whose claim to mathematical fame, it may be said in passing, is that of a circle-squarer.[1] The nominalism of the present-day mathematician consists in treating the objects of his investigation and the relations between them as mere symbols. He then states his propositions, in effect, in the following form: If there exist any objects in the physical or mental world with relations among themselves which satisfy the conditions which I have laid down for my symbols, then such and such facts will be true concerning them.

It will be seen that, according to Peirce's view, the mathematician *as such* is in no wise concerned with the source

[1] Hobbes practically obtains as the ratio of a circumference to its diameter the value $\sqrt{10}$. Cf. for instance Molesworth's edition of Hobbes's English Works, vol. VII, p. 431.

of his premises or with their harmony or lack of harmony with any part of the external world. He does not even assert that any objects really exist which correspond to his symbols. Mathematics may therefore be truly said to be the most abstract of all sciences, since it does not deal directly with reality.[1]

This, then, is Peirce's definition of mathematics. Its advantages in the direction of unifying our conception of mathematics and of assigning to it a definite place among the other sciences are clear. What are its disadvantages? I can see only two. First that, as has been already remarked, the idea of drawing necessary conclusions is a slightly vague and shifting one. Secondly, that it lays exclusive stress on the rigorous logical element in mathematics and ignores the intuitional and other non-rigorous tendencies which form an important element in the great bulk of mathematical work concerning which I shall speak in greater detail later.

IV. *Geometry an Experimental Science*

Some of you will also regard it as an objection that there are subjects which have almost universally been regarded as branches of mathematics but are excluded by this definition. A striking example of this is geometry, I mean the science of the actual space we live in; for though geometry is, according to Peirce's definition, preëminently a mathematical science, it is not exclusively so. Until a system of axioms is established mathematics cannot begin its work. Moreover, the actual perception of spatial relations, not merely in simple cases but in the appreciation of complicated theorems, is an essential element in geometry which has no relation to mathematics as Peirce understands the

[1] Cf. the very interesting remarks along this line of C. S. Peirce in *The Monist*, vol. VII, pp. 23-24.

term. The same is true, to a considerable extent, of such subjects as mechanical drawing and model-making, which involve, besides small amounts of physics and mathematics, mainly non-mathematical geometry. Moreover, although the mathematical method is the traditional one for arriving at the truth concerning geometric facts, it is not the only one. Direct appeal to the intuition is often a short and fairly safe cut to geometric results; and on the other hand experiments may be used in geometry, just as they are used every day in physics, to test the truth of a proposition or to determine the value of some geometric magnitude.[1]

We must, then, admit, if we hold to Peirce's view, that there is an independent science of geometry just as there is an independent science of physics, and that either of these *may* be treated by mathematical methods. Thus geometry becomes the simplest of the natural sciences, and its axioms are of the nature of physical laws, to be tested by experience and to be regarded as true only within the limits of error of observation. This view, while it has not yet gained universal recognition, should, I believe, prevail, and geometry be recognized as a science independent of mathematics, just as psychology is gradually being recognized as an independent science and not as a branch of philosophy.

The view here set forth, according to which geometry is an experimental science like physics or chemistry, has been held ever since Gauss's time by almost all the leading mathematicians who have been conversant with non-Euclidean geometry.[2] Recently, however, Poincaré has thrown the weight of his great authority against this view,[3] claim-

[1] I am thinking of measurements and observations made on accurately constructed drawings and models A famous example is Galileo's determination of the area of a cycloid by cutting out a cycloid from a metallic sheet and weighing it.

[2] Gauss, Riemann, Helmholtz are the names which will carry perhaps the greatest weight.

[3] Cf. *La Science et l'Hypothèse.* Paris, 1903.

ing that the experiments by which it is sought to test the truth of geometric axioms are really not geometrical experiments at all but physical ones, and that any failure of these experiments to agree with the ordinary geometrical axioms could be explained by the inaccuracy of the *physical* laws ordinarily assumed. There is undoubtedly an important element of truth here. Every experiment depends for its results not merely on the law we wish to test, but also on other laws which for the moment we assume to be true. But, if we prefer, we may, in many cases, assume as true the law we were before testing and our experiment will then serve to test some of the remaining laws. If, then, we choose to stick to the ordinary Euclidean axioms of geometry in spite of what any future experiments may possibly show, we can do so, but at the cost, perhaps, of our present simple physical laws, not merely in one branch of physics but in several. Poincaré's view[1] is that it will always be expedient to preserve simple geometric laws at all costs an opinion for which I fail to see sufficient reason.

V. *Kempe's Definition*

Let us now turn from Peirce's method of defining mathematics to Kempe's, which, however, I shall present to you in a somewhat modified form.[2] The point of view adopted here is to try to define mathematics, as other sciences are defined, by describing the objects with which it deals. The diversity of the objects with which mathematics is ordinarily supposed to deal being so great, the first step must be to divest them of what is unessential for the mathematical treatment, and to try in this way to discover their common and characteristic element.

[1] L. c. chapter v. In particular, p. 93.

[2] Kempe has set forth his ideas in rather popular form in the *Proceedings of the London Mathematical Society*, vol. XXVI (1894), p. 5; and in *Nature*, vol. XLIII (1890), p. 156, where references to his more technical writings will be found.

The first point on which Kempe insists is that the objects of mathematical discussion, whether they be the points and lines of geometry, the numbers real or complex of algebra or analysis, the elements of groups or anything else, are always individuals, infinite in number perhaps, but still distinct individuals. In a particular mathematical investigation we may, and usually do, have several different kinds of individuals; as for instance, in elementary plane geometry, points, straight lines, and circles. Furthermore, we have to deal with certain relations of these objects to one another. For instance, in the example just cited, a given point may or may not lie on a given line; a given line may or may not touch a given circle; three or more points may or may not be collinear, etc. This example shows how in a single mathematical problem a large number of relations may be involved, relations some of which connect two objects, others three, etc. Moreover these relations may connect like or they may connect unlike objects; and finally the order in which the objects are taken is not by any means immaterial in general, as is shown by the relation between three points which states that the third is collinear with and lies between the first two.

But even this is not all; for, besides these objects and relations of various kinds, we often have *operations* by which objects can be combined to yield another object, as, for instance, addition or multiplication of numbers. Here the objects combined and the resulting object are all of the same kind, but this is by no means necessary. We may, for instance, consider the operation of combining two points and getting the perpendicular bisector of the line connecting them; or we may combine a point and a line and get the perpendicular dropped from the point on the line.

These few examples show how diverse the relations and

operations, as well as the objects of methamatics, seem at first sight to be. Out of this apparent diversity it is not difficult to obtain a very great uniformity by simply re-stating the facts in a little different language. We shall find it convenient to indicate that the objects $a, b, c, \ldots\ldots$, taken in the order named, satisfy a relation R by simply writing $R(a, b, c, \ldots)$, where it should be understood that among the objects $a, b, c, \ldots$ the same object may occur a number of times. On the other hand, if two objects a and b are combined to yield a third object c, we may write $a \circ b = c$,[1] where the symbol o is characteristic of the special operation with which we are concerned.

Let us first notice that the equation $a \circ b = c$ denotes merely that the three objects a, b, c bear a certain relation to one another, say $R(a, b, c)$. In other words the idea of an operation or law of combination between the objects we deal with, however convenient and useful it may be as a matter of notation, is essentially merely a way of expressing the fact that the objects combined bear a certain relation to the object resulting from their combination. Accordingly, in a purely abstract discussion like the present, where questions of practical convenience are not involved, we need not consider such rules of combination.[2]

Furthermore, it is easy to see that when we speak of objects of different *kinds*, as, for instance, the points and

[1] I speak here merely of dyadic operations,—*i. e.*, of operations by which two objects are combined to yield a third,—these being by far the most important as well as the simplest. What is said, however, obviously applies to operations by which any number of objects are combined.

[2] Even from the point of view of the technical mathematician it may sometimes be desirable to adopt the point of view of a relation rather than that of an operation. This is seen, for instance, in laying down a system of postulates for the theory of abstract groups (cf., for example, Huntington, *Bulletin of the American Mathematical Society*, June, 1902), where the postulate:

If a and b belong to the class, $a \circ b$ belongs to the class,

which in this form looks indecomposable, immediately breaks up, when stated in the relational form, into the following two:

1. If a and b belong to the class, there exists an element c of the class such that $R(a, b, c)$.

2. If a, b, c, d belong to the class, and if $R(a, b, c)$ and $R(a, b, d)$, then $c = d$.

lines of geometry, we are introducing a notion which can very readily be expressed in our relational notation. For this purpose we need merely to introduce a further relation which is satisfied by two or more objects when and only when they are of the same "kind."

Let us turn finally to the relations themselves. It is customary to distinguish here between dyadic relations, triadic relations, etc., according as the relation in question connects two objects, three objects, etc. There are, however, relations which may connect any number of objects, as, for instance, the relation of collinearity which may hold between any number of points. Any relation holds for certain ordered groups of objects but not for others, and it is in no way *necessary* for us to fix our attention on the fact, if it be true, that the number of objects in all the groups for which a particular relation holds is the same. This is the point of view we shall adopt, and we shall relegate the property that a relation is dyadic, triadic, etc., to the background along with the various other properties relations may have,[1] all of which must be taken account of in the proper place.

We are thus concerned in any mathematical investigation, from our present point of view, with just two conceptions: first a set, or as the logicians say, a *class* of objects $a, b, c, \ldots$; and secondly a class of relations $R, S, T, \ldots$. We may suppose these objects divested of any qualitative, quantitative, spatial, or other attributes which they may have had, and regard them merely as satisfying or not satisfying the relations in question, where, again, we are wholly indifferent to the nature which these relations originally had. And now we are in a position to state what I conceive to be really the essential point in Kempe's defini-

[1] For instance, the property of symmetry. A relation is said to be symmetrical if it holds or fails to hold independently of the *order* in which the objects are taken.

tion of mathematics, although I have omitted one of the points on which he insists most strongly,[1] by saying:

If we have a certain class of objects and a certain class of relations, and if the only questions which we investigate are whether ordered groups of these objects do or do not satisfy the relations, the results of the investigation are called mathematics.

It is convenient to have a term to designate a class of objects associated with a class of relations between these objects. Such an aggregate we will speak of as a *mathematical system.* If now we have two different mathematical systems, and if a one-to-one correspondence can be set up between the two classes of objects, and also between the two classes of relations in such a way that whenever a certain ordered set of objects of the first system satisfies a relation of that system, the set consisting of the corresponding objects of the second system satisfies the corresponding relation of that system, and *vice versa,* then it is clear that the two systems are, from our present point of view, mathematically equivalent, however different the nature of the objects and relations may be in the two cases.[2] To use a technical term, the two systems are *simply isomorphic.*[3]

It will be noticed that in the definition of mathematics just given nothing is said as to the method by which we are to ascertain whether or not a given relation holds between the objects of a given set. The method used may

[1] Namely, that the only relation that need be considered is that of being "indistinguishable," *i. e.,* a symmetrical and transitive relation between two groups of objects.

[2] The point of view here brought out, including the term isomorphism, was first developed in a special case,—the theory of groups.

[3] Inasmuch as the relations in a mathematical system are themselves objects, we may, if we choose, take our class of objects so as to include these relations as well as what we called objects before, some of which, we may remark in passing, may themselves be relations. Looked at from this point of view, we need one additional relation which is now the only one which we explicitly call a relation. If we denote this relation by inclosing the objects which satisfy it in parentheses, then if the relation denoted before by $R(a, b)$ is satisfied, we should now write (R, a, b), whereas we should *not* have (a, R, b) (S, R, a, b), etc. Thus we see that any mathematical system may be regarded as consisting of a class of objects and a *single* relation between them.

be a purely empirical one, or it may be partly or wholly deductive. Thus, to take a very simple case, suppose our class of objects to consist of a large number of points in a plane and suppose the only relation between them with which we are concerned is that of collinearity. Then, if the points are given us by being marked in ink on a piece of white paper, we can begin by taking three pins, sticking them into the paper at three of the points; then, by sighting along them, we can determine whether or not these points are collinear. We can do the same with other groups of three points, then with all groups of four points, etc. The same result can be obtained with much less labor if we make use of certain simple properties which the relation of collinearity satisfies, properties which are expressed by such propositions as:

$R(a, b, c)$ implies $R(b, a, c)$,

$R(a, b, c, d)$ implies $R(a, b, c)$,

$R(a, b, c)$ and $R(a, b, d)$ together imply $R(a, b, c, d)$, etc.

By means of a small number of propositions of this sort it is easy to show that no empirical observations as to the collinearity of groups of more than three points need be made, and that it may not be necessary to examine even all groups of three points. Having made this relatively small number of observations, the remaining results would be obtained deductively. Finally, we may suppose the points given by their coördinates, in which case the complete answer to our question may be obtained by the purely deductive method of analytic geometry.

According to the modified form of Kempe's definition which I have just stated, mathematics is not necessarily a deductive science. This view, while not in accord with the prevailing ideas of mathematicians, undoubtedly has its advantages as well as its dangers. The non-deductive pro-

cesses, of which I shall have more to say presently, play too important a part in the life of mathematics to be ignored, and the definition just given has the merit of not excluding them. It would seem, however, that the definition in the form just given is too broad. It would include, for instance, the determination by experimental methods of what pairs of chemical compounds of the known elements react on one another when mixed under given conditions.

VI. *Axioms and Postulates. Existence Theorems*

If, however, we restrict ourselves to exact or deductive mathematics, it will be seen that Kempe's definition becomes coextensive with Peirce's. Here, in order to have a starting-point for deductive reasoning, we must assume a certain number of facts or *primitive propositions* concerning any mathematical system we wish to study, of which all other propositions will be necessary consequences.[1] We touch here on a subject whose origin goes back to Euclid and which has of late years received great development, primarily at the hands of Italian mathematicians.[2]

It is important for us to notice at this point that not merely these primitive propositions but all the propositions of mathematics may be divided into two great classes. On the one hand, we have propositions which state that certain specified objects satisfy certain specified relations. On the other hand are the *existence theorems*, which state that there exist objects satisfying, along with certain specified objects, certain specified relations.[3] These two classes of

[1] These primitive propositions may be spoken of as *axioms* or *postulates*, according to the point of view we wish to take concerning their source, the word axiom, which has been much misused of late, indicating an intuitional or empirical source.

[2] Peano, Pieri, Padoa, Burali-Forti. We may mention here also Hilbert, who, apparently without knowing of the important work of his Italian predecessors, has also done valuable work along these lines.

[3] Or we might conceivably have existence theorems which state that there exist relations which are satisfied by certain specified objects; or these two kinds of existence theorems might be combined.

propositions are well known to logicians and are designated by them *universal* and *particular* propositions respectively.[1] It is only during the last fifty years or so that mathematicians have become conscious of the fundamental importance in their science of existence theorems, which until then they had frequently assumed tacitly as they needed them, without always being conscious of what they were doing.

It is sometimes held by non-mathematicians that if mathematics were really a purely deductive science, it could not have gained anything like the extent which it has without losing itself in trivialities and becoming, as Poincaré puts it, a vast tautology.[2] This view would doubtless be correct if all primitive propositions were universal propositions. One of the most characteristic features of mathematical reasoning, however, is the use which it makes of auxiliary elements. I refer to the auxiliary points and lines in proofs by elementary geometry, the quantities formed by combining in various ways the numbers which enter into the theorems to be proved in algebra, etc. Without the use of such auxiliary elements mathematicians would be incapable of advancing a step; and whenever we make use of such an element in a proof, we are in reality using an existence theorem.[3] These existence theorems need not, to be sure, be among the primitive propositions; but if not, they must be deduced from primitive propositions some of which are existence theorems, for it is clear that an existence theorem cannot be deduced from universal proposi-

[1] "All men are mortals" is a standard example of a universal proposition; while as an illustration of a particular proposition is often given: "Some men are Greeks." That this is really an existence theorem is seen more clearly when we state it in the form: "There exists at least one man who is a Greek."

[2] Cf. *La Science et l'Hypothèse*, p. 10.

[3] Even when in algebra we consider the sum of two numbers $a + b$, we are using the existence theorem which says that, any two numbers a and b being given, there exists a number c which stands to them in the relation which we indicate in ordinary language by saying that c is the sum of a and b.

tions alone.[1] Thus it may fairly be said that existence theorems form the vital principle of mathematics, but these in turn, it must be remembered, would be impotent without the material basis of universal propositions to work upon.

VII. *Russell's Definition*

We have so far arrived at the view that exact mathematics is the study by deductive methods of what we have called a mathematical system, that is, a class of objects and a class of relations between them. If we elaborate this position in two directions we shall reach the standpoint of Russell.[2]

In the first place Russell makes precise the term *deductive method* by laying down explicitly a list of logical conceptions and principles which alone are to be used; and, secondly, he insists,[3] on the contrary, that no mathematical system, to use again the technical term introduced above, be studied in pure mathematics whose existence cannot be established solely from the logical principles on which all mathematics is based. Inasmuch as the development of mathematics during the last fifty years has shown that the existence of most, if not all the mathematical systems which have proved to be important can be deduced when once the existence of positive integers is granted, the point about which interest must centre here is the proof, which Russell attempts, of the existence of this latter system.[4] This

[1] The power which resides in the method of mathematical induction, so called, comes from the fact that this method depends on an existence theorem. It is, however, not the only fertile principle in mathematics as Poincaré would have us believe (cf. *La Science et l'Hypothèse*). In fact there are great branches of mathematics, like elementary geometry, in which it takes little or no part.

[2] *The Principles of Mathematics*, Cambridge, England, 1903.

[3] In the formal definition of mathematics at the beginning of the book this is not stated or in any way implied; and yet it comes out so clearly throughout the book that this is a point of view which the author regards as essential, that I have not hesitated to include it as a part of his definition.

[4] Cf. also Burali-Forti, *Congrès internationale de philosophie*. Paris, vol. III, p. 289.

proof will necessarily require that, among the logical principles assumed, existence theorems be found. Such theorems do not seem to be explicitly stated by Russell, the existence theorems which make their appearance further on being evolved out of somewhat vague philosophical reasoning. There are also other reasons, into which I cannot enter here, why I am not able to regard the attempt made in this direction by Russell as completely successful.[1] Nevertheless, in view of the fact that the system of finite positive integers is necessary in almost all branches of mathematics (we cannot speak of a triangle or a hexagon without having the numbers three and six at our disposal), it seems extremely desirable that the system of logical principles which we lay at the foundation of all mathematics be assumed, if possible, broad enough so that the existence of positive integers—at least finite integers—follows from it; and there seems little doubt that this can be done in a satisfactory manner. When this has been done we shall perhaps be able to regard, with Russell, pure mathematics as consisting exclusively of deductions "by logical principles from logical principles."

VIII. *The Non-Deductive Elements in Mathematics*

I fear that many of you will think that what I have been saying is of an extremely one-sided character, for I have insisted merely on the rigidly deductive form of reasoning used and the purely abstract character of the objects considered in mathematics. These, to the great majority of mathematicians, are only the dry bones of the science. Or, to change the simile, it may perhaps be said that instead of inviting you to a feast I have merely shown you the empty dishes and explain how the feast would be served if only the

[1] Russell's unequivocal repudiation of nominalism in mathematics seems to me a serious if not an insurmountable barrier to progress.

dishes were filled.[1] I fully agree with this opinion, and can only plead in excuse that my subject was the *fundamental* conceptions and methods of mathematics, not the infinite variety of detail and application which give our science its real vitality. In fact I should like to subscribe most heartily to the view that in mathematics, as elsewhere, the discussion of such fundamental matters derives its interest mainly from the importance of the theory of which they are the so-called foundations.[2] I like to look at mathematics almost more as an art than a science; for the activity of the mathematician, constantly creating as he is, guided though not controlled by the external world of the senses, bears a resemblance, not fanciful I believe but real, to the activity of an artist, of a painter let us say. Rigorous deductive reasoning on the part of the methematician may be likened here to technical skill in drawing on the part of the painter. Just as no one can become a good painter without a certain amount of this skill, so no one can become a mathematician without the power to reason accurately up to a certain point. Yet these qualities, fundamental though they are, do not make a painter or a mathematician worthy of the name, nor indeed are they the most important factors in the case. Other qualities of a far more subtle sort, chief among which in both cases is imagination, go to the making of the good artist or good mathematician. I must content myself merely by recalling to you this somewhat vague and difficult though interesting field of speculation which arises when we attempt to attach *value* to

[1] Notice that just as the empty dishes could be filled by a great variety of viands, so the empty symbols of mathematics can be given meanings of the most varied sorts.

[2] Cf. the following remark by Study, *Jahresbericht der deutschen Mathematiker-Vereinigung*, vol. XI (1902), p. 313:
"So wertvoll auch Untersuchungen über die systematische Stellung der mathematischen Grundbegriffe sind . . . *wertvoller* ist doch noch der materielle Inhalt der einzelnen Disciplinen, um dessentwillen allein ja derartige Untersuchungen überhaupt Zweck haben. . . ."

mathematical work, a field which is familiar enough to us all in the analogous case of artistic or literary criticism.

We are in the habit of speaking of logical rigor and the consideration of axioms and postulates as the foundations on which the superb structure of modern mathematics rests; and it is often a matter of wonder how such a great edifice can rest securely on such a small foundation. Moreover, these foundations have not always seemed so secure as they do at present. During the first half of the nineteenth century certain mathematicians of a critical turn of mind—Cauchy, Abel, Weierstrass, to mention the greatest of them—perceived to their dismay that these foundations were not sound, and some of the best efforts of their lives were devoted to strengthening and improving them. And yet I doubt whether the great results of mathematics seemed less certain to any of them because of the weakness they perceived in the foundations on which these results are built up. The fact is that what we call mathematical rigor is merely one of the foundation stones of the science; an important and essential one surely, yet not the only thing upon which we can rely. A science which has developed along such broad lines as mathematics, with such numerous relations of its parts both to one another and to other sciences, could not long contain serious error without detection. This explains how, again and again, it has come about, that the most important mathematical developments have taken place by methods which cannot be wholly justified by our present canons of mathematical rigor, the logical "foundation" having been supplied only long after the superstructure had been raised. A discussion and analysis of the non-deductive methods which the creative mathematician really uses would be both interesting and instructive. Here I must content myself with the enumeration of a few of them.

First and foremost there is the use of intuition, whether geometrical, mechanical, or physical. The great service which this method has rendered and is still rendering to mathematics both pure and applied is so well known that a mere mention is sufficient.

Then there is the method of experiment; not merely the physical experiments of the laboratory or the geometrical experiments I had occasion to speak of a few minutes ago, but also arithmetical experiments, numerous examples of which are found in the theory of numbers and in analysis. The mathematicians of the past frequently used this method in their printed works. That this is now seldom done must not be taken to indicate that the method itself is not used as much as ever.

Closely allied to this method of experiment is the method of analogy, which assumes that something true of a considerable number of cases will probably be true in analogous cases. This is, of course, nothing but the ordinary method of induction. But in mathematics induction may be employed not merely in connection with the experimental method, but also to extend results won by deductive methods to other analogous cases. This use of induction has often been unconscious and sometimes overbold, as, for instance, when the operations of ordinary algebra were extended without scruple to infinite series.

Finally there is what may perhaps be called the method of optimism, which leads us either willfully or instinctively to shut our eyes to the possibility of evil. Thus the optimist who treats a problem in algebra or analytic geometry will say, if he stops to reflect on what he is doing: "I know that I have no right to divide by zero; but there are so many other values which the expression by which I am dividing might have that I will assume that the Evil One has not thrown a zero in my denominator this time." This

method, if a proceeding often unconscious can be called a method, has been of great service in the rapid development of many branches of mathematics, though it may well be doubted whether in a subject as highly developed as is ordinary algebra it has not now survived its usefulness.[1]

While no one of these methods can in any way compare with that of rigorous deductive reasoning as a method upon which to base mathematical results, it would be merely shutting one's eyes to the facts to deny them their place in the life of the mathematical world, not merely of the past but of to-day. There is now, and there always will be room in the world for good mathematicians of every grade of logical precision. It is almost equally important that the small band whose chief interest lies in accuracy and rigor should not make the mistake of despising the broader though less accurate work of the great mass of their colleagues; as that the latter should not attempt to shake themselves wholly free from the restraint the former would put upon them. The union of these two tendencies in the same individuals, as it was found, for instance, in Gauss and Cauchy, seems the only sure way of avoiding complete estrangement between mathematicians of these two types.

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[1] Cf. the very suggestive remarks by Study, *Jahresbericht d. Deutschen Mathematiker-Vereinigung,* vol. XI (1902), p. 100, footnote, in which it is pointed out how rigor, in cases of this sort, may not merely serve to increase the correctness of the result, but actually to suggest new fields for mathematical investigation.

UTILITARIAN SCIENCE

BY DAVID STARR JORDAN

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IT falls to my lot to-day, to discuss very briefly, in accordance with the Programme of this Congress, some of the common features of utilitarian science, with a word as to present and future lines of investigation or instruction in some of those branches of the applications of knowledge which have been assigned to the present division.

Applied science cannot be separated from pure science; for pure science may develop at any quarter the greatest and most unexpected economic values; while on the other hand, the application of knowledge must await the acquisition of knowledge before any high achievement can be reached. For these reasons, the classification adopted in the present Congress, or any other classification of sciences into utilitarian science and other forms of science, must be incomplete and even misleading. Whatever is true is likely some time to prove useful, and all error is likely to prove some time disastrous. From the point of view of the development of the human mind, all truth is alike useful, and all error is alike mischievous.

In point of development, pure science must precede utilitarian science. Historically, this seems to be not true; for the beginnings of science in general, as alchemy, astrology, and therapeutics, seem to have their origin in the desire for the practical results of knowledge. Men wanted to acquire gold, to save life, to forecast the future, not for knowledge's sake, but for the immediate results of success in these directions. But even here accurate knowledge must precede any success in its application, and accuracy of knowledge is all that we mean by pure science. Moreover, as through the ages the representatives of the philosophies of the day, the *a priori* explanations of the universe, were bitterly and personally hostile to all inductive conclusions based on the study of base matter, men of science were forced to disguise their work under a utilitarian cloak. This is more or less true even to this day, and the greatest need of utilitarian science is still, as a thousand years ago, that this cloak should be thrown off, and that a larger and stronger body of workers in pure science should be developed to give the advance in real knowledge on which the thousands of ingenious and noble applications to utilitarian ends must constantly depend.

It is a fundamental law of psychology that thought tends to pass over into action. Applied science is knowledge in action. It is the flower of that highest philanthropy of the ages by which not even thought exists for itself alone, but must find its end in the enlargement of human control over matter and force or the amelioration of the conditions of human life.

The development of all science has been a constant struggle, a struggle of fact against philosophy, of instant impressions against traditional interpretations, of truth against "make-believe." For men are prone to trust a theory rather than a fact; a fact is a single point of contact; a theory

is a circle made of an infinite number of points, none of them, however, it may be, real points of contact.

The history of the progress of science is written in human psychology rather than in human records. It is the struggle of the few realities or present sense-impressions against the multitude of past impressions, suggestions, and explanations. I have elsewhere said that the one great discovery of the nineteenth century—forestalled many ages before—was that of the reality of external things. Men have learned to trust a present fact or group of facts, however contradictory its teachings, as opposed to tradition and philosophy. From this trust in the reality of the environment of matter and force, whatever these may be, the great fabric of modern science has been built up. Science is human experience of contact with environment tested, set in order, and expressed in terms of other human experience. Utilitarian science is that part of all this knowledge which we can use in our lives, in our business. What is pure science to one is applied science to another. The investigation of the laws of heredity may be strictly academic to us of the university, but they are utilitarian as related to the preservation of the nation or to the breeding of pigs. In the warfare of science the real in act and motive has been persistently substituted for the unreal. Men have slowly learned that the true glory of life lies in its wise conduct, in the daily act of love and helpfulness, not in the vagaries fostered by the priest or in the spasms of madness which are the culmination of war. To live here and now as a man should live constitutes the ethics of science, and this ideal has been in constant antithesis to the ethics of ecclesiasticism, of asceticism, and of militarism.

The physical history of the progress of science has been a struggle of thinkers, observers, and experimenters against the dominant forces of society. It has been a continuous

battle, in which the weaker side, in the long run, is winner, having the strength of the earth behind. It has been incidentally a conflict of earth-born knowledge with opinions of men sanctioned by religion; of present fact with preëstablished system, visibly a warfare between inductive thought and dogmatic theology.

The real struggle, as already indicated, lies deeper than this. It is the effort of the human mind to relate itself to realities in the midst of traditions and superstitions, to realize that nature never contradicts herself, is always complex, but never mysterious. As a final result all past systems of philosophy, perhaps all possible systems, have been thrown back into the realm of literature, of poetry, no longer controlling the life of action, which rests on fact.

This conflict of tendencies in the individual has become a conflict among individuals as each is governed by a dominant impulse. The cause of tradition becomes that of theology;—for men have always claimed a religious sanction for their own individual bit of cosmic philosophy. Just as each man in his secret heart, the centre of his own universe, feels himself in some degree the subject of the favor of the mysterious unseen powers, so does society in all ages find a mystic or divine warrant for its own attitude towards life and action, whatever that may be.

The nervous system of man, inherited from that of the lower animals, may be regarded as primarily a means of making locomotion safe. The reflex action of the nerve centre is the type of all mental processes. The sensorium, or central ganglion, receives impressions from the external world representing, in a way, various phases of reality. The brain has no source of knowledge other than sensation. All human knowledge comes through human experience. The brain, sitting in darkness, has the primary function of converting sensory impressions into impulses to

action. To this end the motor nerves carry impulses outward to the muscles. The higher function of nerve-action, which we call the intellect, as distinguished from simple reflex action and from instinct, is the choice among different responses to the stimulus of external realities. As conditions of life become more complex, the demands of external realities become more exacting. It is the function of the intellect to consider and of the mind to choose. The development of the mind causes and permits complexity in external relations. Safety in life depends on choosing the right response to external stimulus. Wrong choice leads to failure or to death.

From the demands of natural selection results the intense practicality of the mental processes. Our senses tell us the truth as to external nature, in so far as such phases of reality have been essential to the life of our ancestors. To a degree, they must have seen "things as they really are," else they should not have lived to continue the generation. Our own individual ancestors through all the ages have been creatures of adequate accuracy of sensation and of adequate power of thought. Were it not so they could not have coped with their environment. The sensations which their brains translated into action contained enough of absolute reality to make action safe. That our own ordinary sensations and our own inductions from them are truthful in their essentials, is proved by the fact that we have thus far safely trusted them. Science differs from common sense mainly in the perfection of its tools. That the instruments of precision used in science give us further phases of reality is shown by the fact that we can trust our lives to them. We find it safer to do so than to trust our unaided senses.

While our senses tell us the truth as to familiar things, as rocks and trees, foods and shelter, friends and enemies,

they do not tell us the whole truth: they go only so far as the demands of ancestral environment have forced them to go. Chemical composition our senses do not show. Objects too small to handle are too small to be seen. Bodies too distant to be reached are never correctly apprehended. Accuracy of sense decreases as the square of the distance increases. Sun and stars, clouds and sky, are in fact very different from what they seem to the senses.

In matters not vital to action, exactness of knowledge loses its importance. Any kind of belief may be safe, if it is not to be carried over into action. It is perfectly safe, in the ordinary affairs of life, for one who does not propose to act on his convictions to believe in witches and lucky stones, imps and elves, astral bodies and odic forces. It is quite as consistent with ordinary living to accept these as objective realities as it is to have the vague faith in microbes and molecules, mahatmas and protoplasm, protective tariffs and manifest destiny, which forms part of the mental outfit of the average American citizen to-day. Unless these conceptions are to be brought into terms of personal experience, unless in some degree we are to trust our lives to them, unless they are to be wrought into action, they are irrelevant to the conduct of life. As they are tested by action, the truth is separated from the falsehood, and the error involved in vague or silly ideas becomes manifest. As one comes to handle microbes, they become as real as bullets or oranges and as susceptible of being manipulated. But the astral body covers only ignorance and ghosts vanish before the electric light.

Memory-pictures likewise arise to produce confusion in the mind. The record of past realities blends readily with the present. Men are gregarious creatures and their speech gives them the power to add to their own individual experiences the concepts and experiences of others. Sugges-

tion and conventionality play a large part in the mental equipment of the individual man.

About the sense-impressions formed in his own brain each man builds up his own subjective universe. Each accretion of knowledge must be cast more or less directly in terms of previous experience. By processes of suggestion and conventionality the ideas of the individual become assimilated to those of the multitude. Thus myths arise to account for phenomena not clearly within the ordinary experiences of life. And in all mythology the unknown is ascribed not to natural forces, but to the action of the powers that transcend nature, that lie outside the domain of the familiar and the real.

It has been plain to man in all ages that he is surrounded by forces stronger than himself, invisible and intangible, inscrutable in their real nature, but terribly potent to produce results. He cannot easily trace cause and effect in dealing with these forces; hence it is natural that he should doubt the existence of relations of cause and effect. As the human will seems capricious because the springs of volition are hidden from observation, so to the unknown will that limits our own we ascribe an infinite caprice. All races of men capable of abstract thought have believed in the existence of something outside themselves whose power is without human limitations. Through the imagination of poets the forces of nature become personified. The existence of power demands corresponding will. The power is infinitely greater than ours; the sources of its action inscrutable: hence man has conceived the unknown first cause as an infinite and unconditioned man. Anthropomorphism in some degree is inevitable, because each man must think in terms of his own experience. Into his own personal universe, all that he knows must come.

Recognition of the hidden but gigantic forces in nature

leads men to fear and to worship them. To think of them either in fear or in worship is to give them human forms.

The social instincts of man tend to crystallize in institutions even his common hopes and fears. An institution implies a division of labor. Hence, in each age and in each race men have been set apart as representatives of these hidden forces and devoted to their propitiation. These men are commissioned to speak in the name of each god that the people worship or each demon the people dread.

The existence of each cult of priests is bound up in the perpetuations of the mysteries and traditions assigned to their care. These traditions are linked with other traditions and with other mystic explanations of uncomprehended phenomena. While human theories of the sun, the stars, the clouds, of earthquakes, storms, comets, and disease, have no direct relation to the feeling of worship, they cannot be disentangled from it. The uncomprehended, the unfamiliar, and the supernatural are one and the same in the untrained human mind; and one set of prejudices cannot be dissociated from the others.

To the ideas acquired in youth we attach a sort of sacredness. To the course of action we follow we are prone to claim some kind of mystic sanction; and this mystic sanction applies not only to acts of virtue and devotion, but to the most unimportant rites and ceremonies; and in these we resent changes with the full force of such conservatism as we possess.

It is against limited and preconceived notions that the warfare of science has been directed. It is the struggle for the realities on the part of the individual man. Ignorance, prejudice, and intolerance, in the long run, are one and the same thing. In some one line, at least, every lofty mind throughout the ages has demanded objective reality. This struggle has been one between science and

theology only because theological misconceptions were entangled with crude notions of other sorts. In the experience of a single human life there is little to correct even the crudest of theological conceptions. From the supposed greater importance of religious opinions in determining the fate of men and nations, theological ideas have dominated all others throughout the ages; and in the nature of things, the great religious bodies have formed the stronghold of conservatism against which the separated bands of science have hurled themselves, seemingly in vain.

But the real essence of conservatism lies not in theology. The whole conflict, as I have already said, is a struggle in the mind of man. From some phase of the warfare of science no individual is exempt. It exists in human psychology before it is wrought in human history. There is no better antidote to bigotry than the study of the growth of knowledge. There is no chapter in history more encouraging than that which treats of the growth of open-mindedness. The study of this history leads religious men to avoid intolerance in the present, through a knowledge of the evils intolerance has wrought in the past. Men of science are spurred to more earnest work by the record that through the ages objective truth has been the final test of all theories and conceptions. All men will work more sanely and more effectively as they realize that no good to religion or science comes from "wishing to please God with a lie."

It is the mission of science to disclose—so far as it goes —the real nature of the universe. Its function is to eliminate, wherever it be found, the human equation. By methods of precision of thought and instruments of precision of observation and experiment, science seeks to make our knowledge of the small, the distant, the invisible, the mys-

terious, as accurate, as practical, as our knowledge of common things. Moreover, it seeks to make our knowledge of common things accurate and precise, that this accuracy and precision may be translated into action. For the ultimate end of science as well as its initial impulse is the regulation of human conduct. Seeing true means thinking right. Right thinking means right action. Greater precision in action makes higher civilization possible. Lack of precision in action is the great cause of human misery; for misery is the inevitable result of wrong conduct. "Still men and nations reap as they have strewn."

A classic thought in the history of applied science is expressed in these words of Huxley: "There can be no alleviation of the sufferings of man except in absolute veracity of thought and action and a resolute facing of the world as it is." "The world as it is" is the province of science. "The God of the things as they are is the God of the highest heaven." And as to the sane man, the world as it is is glorious, beautiful, harmonious, and divine, so will science, our tested and ordered knowledge of it, be the inspiration of art, poetry, and religion.

Pure science and utilitarian science merge into each other at every point. They are one and the same thing. Every new truth can be used to enlarge human power or to alleviate human suffering. There is no fact so remote as to have no possible bearing on human utility. Every new conception falls into the grasp of that higher philanthropy which rests on the comprehension of the truths of science. For science is the flower of human altruism. No worker in science can stand alone. None counts for much who tries to do so. He must enter into the work of others. He must fit his thought to theirs. He must stand on the shoulders of the past, and must crave the help of the future. The past has granted its assistance to the fullest degree

of the most perfect altruism. The future will not refuse; and, in return, whatever knowledge it can take for human uses, it will choose in untrammeled freedom. The sole line which sets off utilitarian science lies in the limitation of human strength and of human life. The single life must be given to a narrow field, to a single strand of truth, following it wherever it may lead. Some must teach, some must investigate, some must adapt to human uses. It is not often that these functions can be united in the same individual. It is not necessary that they should be united; for art is long, though life is short, and for the next thousand years science will be still in its infancy. We stand on the threshold of a new century; a century of science; a century whose discoveries of reality shall far outweigh those of all centuries which have preceded it; a century whose glories even the most conservative of scientific men dare not try to forecast. And this twentieth century is but one—the least, most likely—of the many centuries crowding to take their place in the line of human development. In each century we shall see a great widening of the horizon of human thought, a great increase of precision in each branch of human knowledge, a great improvement in the conditions of human life, as enlightenment and precision come to be controlling factors in human action.

In the remaining part of this address I shall discuss very briefly some salient features of practice, investigation, and instruction in those sciences which in the scheme of classification of this Congress have been assigned to this division. In this discussion I have received the invaluable aid of a large number of my colleagues in scientific work, and from their letters of kindly interest I have felt free to make some very interesting quotations. To all these gentlemen (a list too long to be given here) from whom I have received aid of this kind, I offer a most grateful acknowledgment.

Engineering

The development of the profession of engineering in America has been the most remarkable feature of our recent industrial as well as educational progress. In this branch of applied science our country has come to the very front, and this in a relatively short time. To this progress a number of distinct forces have contributed. One lies in the temperament of our people, their native force, and their tendency to apply knowledge to action. In practical life the American makes the most of all he knows. Favoring this is the absence of caste feeling. There is no prejudice in favor of the idle man. Only idlers take the members of the leisure class seriously. There is, again, no social discrimination against the engineer as compared with the other learned professions. The best of our students become working engineers without loss of social prestige of any sort. Another reason is found in the great variety of industrial openings in America, and still another in the sudden growth of American colleges into universities, and universities in which both pure and applied sciences find a generous welcome. For this the Morrill Act, under which each state has developed a technical school, under federal aid, is largely responsible. In the change from the small college of thirty years ago, a weak copy of English models, to the American university of to-day many elements have contributed. Among these is the current of enlightenment from Germany, and at the same time the influence of far-seeing leaders in education. Notable among these have been Tappan, Eliot, Agassiz, and White. To widen the range of university instruction so as to meet all the intellectual, esthetic, and industrial needs of the ablest men is the work of the modern university. To do this work is to give a great impetus to pure and to applied science.

Two classes of men come to the front in the development of engineering: the one, men of deep scientific knowledge, to whom advance of knowledge is due, the other the great constructive engineers; men who can work in the large and can manage great enterprises with scientific accuracy and practical success. Everywhere the tendency in training is away from mere craftsmanship and towards power of administration. The demands of the laboratory leave less and less time for the shop. "Two classes of students," says a correspondent, "should be encouraged in our universities: First, the man whose scientific attainments are such that he will be able to devolop new and important processes, the details of which may be directly applied. This type of man is the scientific engineer. The other is the so-called practical man, who will not only actually carry on engineering work, but may be called on to manage large enterprises. If his temperament and ability are such as to give him a thorough command of business methods and details, while he is in addition a good engineer, he will find a field of great usefulness before him on leaving the university. The university should encourage young men to undertake the general executive work necessary to handling men and in the many details of large enterprises. The successful man of this character is necessarily a leader, and the university should recognize that such a man can be of great influence in the world, if he is thoroughly and broadly educated."

"We need," says another correspondent, "men possessing a better general training than most of those now entering and leaving our engineering schools. We need more thoroughly trained teachers of engineering, men who combine theoretical training with a wide and constantly increasing experience, men who can handle the factors of theory, practice, and economics."

"Technical education," says another correspondent, "should look beyond the individual to the aggregate, and should aim to shape its activities so as to develop at the maximum number of points sympathetic and helpful relations with the industrial and engineering interests of the state. This means careful and steady effort towards the coördination of the activities of the technical school with the general condition of industry and engineering as regards its raw materials, its constructive and productive operations, its needs and demands with regard to personnel, and its actual or potential trend of progress."

The coming era in engineering is less a period of discovery and invention than of application on a large scale of principles already known. Greater enterprises, higher potentialities, freer use of forces of nature, all these are in the line of engineering progress.

"The realm of physical science," says a correspondent, "has become to the practical man a highly improved agricultural land, whereas in earlier days it was a virgin country possessing great possibilities and exacting but little in the way of economic treatment."

In all forms of engineering, practice is changing from day to day; the principles remain fixed. In electricity, for example, the field of knowledge "extends far beyond the direct limits or needs of electrical engineers."

"The best criticism as to engineering education came formerly almost entirely from professors of science and engineering. To-day the greatest and most wholesome source of such criticism comes from those engaged in practical affairs. We have begun a régime wherein coördinated theory and practice will enter into the engineering training of young men to a far greater and more profitable extent than ever before."

"The marvelous results in the industrial world of to-day,"

says a correspondent, "are due largely to the spirit of 'usefulness, activity, and coöperation' that exists in each community of interests and which actuates men employing the means which applied science has so bountifully accorded. I know of no greater need of engineering education in our country to-day than that its conduct in each institution should be characterized by the same spirit of usefulness, activity, and coöperation."

In mining, as in other departments of engineering, we find in the schools the same growing appreciation of the value of training at once broad, thorough, and practical, and the same preference for the university-trained engineer over the untrained craftsman.

The head of a great mining firm in London writes me that "for our business, what we desire are young men of good natural qualifications, thoroughly trained theoretically without any so-called practical knowledge unless this knowledge has been gained by employment in actual works."

On the pay-roll of this English firm I find that five men receive salaries of more than $20,000. All these are graduates of technical departments of American universities. Seventeen receive from $6000 to $20,000. Nine of these were trained in American universities, one in Australia, and two in England, while five have risen from the ranks.

In the lower positions, most have been trained in Australia, a few in England, while in positions bearing a salary of less than $2500 most have risen from the ranks.

"Given men of equal qualifications," says the director of this firm, "the man of technical training is bound to rise to the higher position because of his greater value to his employer. As a rule, also, men who have been technically trained are, by virtue of their education, men who are endowed with a professional feeling which does not to the same extent exist among those men who have risen from

the rank and file. They are therefore more trustworthy, and especially in mining work, where premium for dishonesty exists, for this qualification alone they are bound to have precedence. We do not by any means wish to disparage the qualifications of many men who have risen from the ranks to eminent positions, but our opinion may be concentrated in the statement that even these men would be better men had they received a thorough technical training."

The progress of chemical engineering is parallel with that in other departments of technology. Yet the appreciation of the value of theoretical training is somewhat less marked, and in this regard our manufacturers seem distinctly behind those of Germany.

"The development of chemical industries in the past history of the United States," says a correspondent, "was seriously delayed by the usually superficial and narrow training of the chemist in the colleges. Thus managers and proprietors came to undervalue the importance of chemical knowledge. The greatest need at present in the development of chemical industries is an adequate supply of chemists of thorough training to teach manufacturers the importance in their business of adequate chemical knowledge. Epoch-making advances in chemical industry will spring from the brain of great chemists, and to insure the production of a few of these, the country must expect to seed lavishly and to fertilize generously the soil from which they spring. Germany has learned the lesson well: other nations cannot long delay."

Agriculture

In the vast range of the applications of science to agriculture, the same general statements hold good. There is, however, no such general appreciation of the value of train-

ing as appears in relation to the various branches of training, and the men of scientific education are mostly absorbed in the many ramifications of the Department of Agriculture and in the state agricultural colleges and experiment stations. There are few illustrations of the power of national coöperation more striking than those shown in the achievements of the Department of Agriculture. I have no time to touch on the varied branches of agricultural research, the study of the chemistry of foods and soils, the practice of irrigation, the fight against adulterations, the fight against noxious insects, and all the other channels of agricultural art and practice. I can only commend the skill and the zeal with which all these lines of effort have been followed.

The art of agriculture is the application of all the sciences. Yet "agricultural education," writes a correspondent, "has not yet reached the dignity of other forms of technical education."

"The endowment of the science of agricultural research in the United States is greater than in any other country. The chief fault to be found is in striving too rapidly for practical applications and in not giving time enough for the fundamental research on which these applications must rest. The proportion of applied agricultural science in agriculture is too great in this country. While we do not need fewer workers in applied agricultural science, we do need more workers who would devote themselves to fundamental research."

Two branches of applied science not specifically noticed in our scheme of classification seem to me to demand a word of notice. One is selective breeding of plants and animals; the other, the artificial hatching of fishes. By the crossing of animals or plants not closely related, a great range of variety appears in the progeny. Some of these

may have one or more of the desirable qualities of either parent. By selection of those possessing such qualities a new race may be formed in a few generations. The practical value of the results of such experiments cannot be over-estimated. Although by no means a modern process, the art of selective breeding is still in its infancy. Its practice promises to take a leading place among the economically valuable applications of science. At the same time, the formation of species of organisms under the hand of man throws constant floods of light on the great questions of heredity, variation, and selection in nature, the problem of the origin of species.

In this connection I may refer to artificial hatching and acclimatization of fishes, the work of the United States Bureau of Fisheries and of the fish commissions of the different states. There are many species of fish, notably those of the salmon family, in which the eggs can be taken and fertilized by artificial processes. These eggs can be hatched in protected waters so that the young will escape many of the vicissitudes of the brook and river, and a thousand young fishes can be sent forth where only a dozen grew before.

Medicine

In the vast field of medicine I can only indicate in a few words certain salient features of medical research, of medical practice, and of medical instruction in America.

In matters of research, the most fruitful line of investigation has been along the line of the mechanism of immunity from contagious diseases. To know the nature of microörganisms and their effect on the tissues is to furnish the means of fighting them. "The first place in experimental medicine to-day," says Dr. W. H. Welch, "is occupied by the problem of immunity." That medicine is

becoming a scientific profession and not a trade is the basis of the growing interest of our physicians in scientific problems, and this again leads to increased success in dealing with matters of health and disease. The discovery of the part played by mosquitoes in the dissemination of malaria, yellow fever, dengue, elephantiasis, and other diseases caused by microörganisms marks an epoch in the study of these diseases. The conquest of diphtheria is another of the features of advance in modern medicine, and another is shown in the great development of surgical skill characteristic of American medical science. But the discoveries of the last decades have been rarely startling or epoch-making. They have rather tended to fill the gaps in our knowledge, and there remain many more gaps to fill, before medical practice can reach the highest point of adequacy. The great need of the profession is still in the direction of research, and research of the character which takes the whole life and energy of the ablest men demands money for its maintenance. We need no more medical colleges for the teaching of the elements. We need schools or laboratories of research for the training of the masters.

In the development of medicine there has been a steady movement away from universal systems and *a priori* principles, on the one hand, and, on the other hand, from blind empiricism, with the giving of drugs with sole reference to their apparent results. The applications of sciences —all sciences which deal with life, with force, and with chemical composition—must enter into the basis of medicine. Hence the insistent demand for better preliminary training before entering on the study of medicine. "Only the genius of the first order," says a correspondent, "can get on without proper schooling in his youth. What our medical investigators in this country most need is a thorough grounding in the sciences, especially physics and chemistry."

The instruction in medicine, a few years ago almost a farce in America, has steadily grown more serious. Laboratory work and clinical experience have taken the place of lectures, the courses have been lengthened, higher preparation for entrance has been exacted, though in almost all our schools these requirements are still far too low, and a more active and original type of teacher has been in demand. Even yet, so far as medical instruction is concerned, the hopeful sign is to be found in progress rather than in achievement. A college course, having as its major subjects the sciences fundamental to medicine, is not too much to exact of a student who aspires to be a physician worthy of our times and of the degree of our universities. First-hand knowledge of real things should be the keynote of all scientific instruction. "Far more effort is now made," writes a correspondent, "in both the preparatory and the clinical branches to give the student a first-hand knowledge of his subject. This tendency has still a long way to travel before it is in danger of being overdone. The practical result of this tendency is that the cost of education per student is greatly increased and the profits of purely commercial schools are thereby threatened. This forms, doubtless, the main source of the objection made by the weaker and less worthy schools to better methods of instruction. We need well-endowed schools of medicine that may carry on their work unhampered by the necessities of a commercial venture. Medical schools now exist in great numbers,—many of them cannot keep up with modern requirements, and necessarily their salvation lies in antagonizing everything in the nature of more ample and more expensive training."

Another correspondent writes, emphasizing the value of biologic studies: "The final comprehension of bodily activity in health and disease depends on knowledge of living

things from ovum to birth, from birth to maturity, and from maturity to old age and death. Anything less than such fundamental knowledge requires constant guessing to fill up the gaps, and guesses are nearly always wrong."

In many regards, even our best schools of medicine seem to show serious deficiencies. The teaching of anatomy is still one of the most costly, as well as least satisfactory, of our lines of work. A correspondent calls attention to the fact that in making anatomy "practical" in our medical schools, "we expended last year $750,000 in the United States, twice the amount expended in Germany, with as a result neither practical anatomy nor scientific achievement." "Anatomy," he continues, "should be made distinctly a university department, on a basis similar to that of physics and chemistry. Unfortunately, university presidents still stand much in the way of the development of anatomy, for many of them seem to think that almost any one who wears the gown is good enough to become a professor of anatomy. Repeatedly have I witnessed the appointment of a know-nothing when a recognized young man might have been had for half the money." Our forces are dissipated, the fear of things scientific has destroyed even the practical in this noble old mother science which is still giving birth to new sciences and to brilliant discoveries.

Among other matters too much neglected are personal hygiene, a matter to which the physician of the past has been notoriously and joyously indifferent. Especially is this true as regards the hygiene of exercise and the misuse of nerve-affecting drugs.

Public sanitation as well deserves more attention. "The demand for adequately trained officers of public health is not what it should be, and our public service as a whole is far below that of European countries. Both public opinion and university authorities are responsible for this condition."

The hygiene of childhood, in which line great advances are made, is still not adequately represented in most of our medical colleges, and the study of psychiatry and nervous disturbances in general is not sufficiently lifted from the realm of quackery. "Not only," says a correspondent, "should psychiatry be taught in every medical school, but it should be taught from a clinical standpoint. Every city in which there are medical schools should have a psychopathic hospital for the reception of all cases of alleged insanity and for their study, treatment, and cure. Such a hospital should contain, also, a laboratory for the study of normal and of pathological psychology. I am convinced that progress in normal psychology will be made chiefly through the study of abnormal conditions, just as physiology has profited so enormously through the work of the pathologist."

A word should be said for veterinary medicine and its achievements of enormous economic value in the control of the contagious diseases of animals. The recent achievements of vaccination against the Southern cattle fever and against tuberculosis, the eradication of the foot and mouth disease among other matters, have demanded the highest scientific knowledge and the greatest skill in its practical application.

Unfortunately, veterinary science lacks in this country adequate facilities for research and instruction. "Practically," says a correspondent, "the veterinary sciences in the United States are leading a parasitic existence. We are dependent almost wholly upon the results of investigation and teaching of European countries, notably Germany and Denmark. The value of the live-stock industry here is so tremendous that almost every state in the Union should have a well-equipped veterinary school supported by public funds. There is but one veterinary school in the United

States that has anything like adequate support." That this is true shows that our farmers and stock-raisers are very far from having an adequate idea of one of the most important of their economic needs.

Economics

We may justify the inclusion of economics among the utilitarian sciences on grounds which would equally include the sciences of ethics and hygiene. It is extremely wise as well as financially profitable to take care of one's health, and still more so to take thought of one's conduct. The science of economics in some degree touches the ethics of nations and the "wealth of nations," a large factor in the happiness of the individuals contained within them, depends on the nation's attitude towards economic truths. Another justification of this inclusion is found in the growing tendency in our country to call on professional economists to direct national operations. On the other hand, our economists themselves are becoming more and more worthy of such trusts. The inductive study of their science brings them into closer contact with men and with enterprises. By this means they become students of administration as well as of economics. They realize the value of individual effort as well as the limitations which bound all sorts of executive work, in a republic. "Only a few years ago," writes a correspondent, "the teachers of economics were far more generally unfavorable critics of government work which interested them. They have become more and more disposed to coöperate at the beginning rather than to condemn at the end. Just as economics has taken a more kindly and hospitable attitude towards politics, so similarly has it towards business, as illustrated in the rapid rise of courses in commerce." The demand for trained economists in public affairs is "compelling the

teachers of economics more and more to seek contact with the men who are grappling face to face with economic problems."

The relation of economic theory to administration is a subject on which there is much diversity of opinion. It is claimed by able authority that "economic science, by becoming ultra-theoretical, has come into far closer touch with practical life than it ever attained before. Laws, the statement of which seems like a refinement of theory, determine the kind of legislation required on the most practical of subjects." On another hand, it is claimed by high authority that our country must have its own political economy. "The generalizations arising solely from the uniformity of human nature are so few that they cannot constitute a science. The classical or orthodox political economy of England was conditioned from start to finish by the political problems it had to face. We are only beginning to acquire our national independence."

Still another view is that "all that has been achieved in the field of economics that is of any value, has been the result of logical analysis applied to the phenomena and experiences of every-day industrial life. The stages of past development can be determined and interpreted only in the light of this analysis. The lesson which the historical economist has never learned, is the importance of that principle, which lies at the bottom of the whole modern theory of evolution, and which was made use of by Lyell and Darwin, namely, the principle that historical changes of the past are to be accounted for by the long continued action of causes which are at this present moment in operation and can be observed and measured at the present day." "This," says my correspondent, "needs saying and re-saying, until it is burned into the minds of all students of economics."

The recent progress of economics in America has lain in part in the development of economic theory by critical and by constructive methods. An important reason for welcoming the exact and critical study of economic theory is this: In the promulgation of imaginary economic principles the social and political charlatan finds his choice field of operation, just as the medical charlatan deals with some universal law of disease and its universal cure. The progress of science in every field discredits these universal principles with their mystical panaceas. There is all the more reason why in politics, as in medicine, those generalizations which deal with necessary laws or actually observed sequence of events should be critically and constructively studied.

In general, however, the progress of economics has followed the same lines as progress in other sciences, through a "minute investigation and the application of principles already discovered or outlined by painstaking inquiry as to facts." This method of work has been especially fruitful in the study of monetary problems, of finance, taxation, and insurance, in the study of labor problems and conditions, in the study of commerce, and in the study of crime and pauperism. In its development economics is, however, many years behind the natural sciences, a condition due to reliance on metaphysical methods and to the inherent difficulty in the use of any other.

"Economics," says a correspondent, "has been less successful than the material sciences in getting rid of the apparatus of metaphysical presumptions. The economist is still too eager to formulate laws that shall disclose the ultimate spiritual meaning of things instead of trying to explain how these things came to pass. He has profited in small degree by those lessons which the progressive evolutionary sciences have driven home in the past in the meth-

ods of thinking of workers in other fields. Our science is still sadly behind the times in its way of handling its subject-matter. The greatest and most important work of economic investigations is to make students see things as they are, to fit young men for the more highly organized business new conditions are ushering in, and give a better appreciation of the problems of government and a better training for participation in them."

Says another correspondent: "Training in research is in fact essential to every technical man. The young technologist will be confronted by new problems not covered by anything in literature or in his past experience. Training in research is training in the art of solving unsolved problems, and the practical man who has had discipline of that kind has a great advantage over his more conventional competitors. The Germans recognize this principle, and behold their marvelous industrial growth. The student in every department of science should be taught to think as well as to do."

The time must come when a man who has no training and no experience in research will not be called educated, whatever may be the range of his erudition. To unfold the secret of power is the true purpose of education.

THE DEVELOPMENT OF MODERN MEDICINE

BY FRANK BILLINGS

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Modern medicine is a composite of the knowledge of many sciences. The last twenty-five years mark the period of the greatest evolution of medicine in its history. The foundation of modern medicine was laid by the labors of hundreds of earnest workers in the field of science during the last three centuries. As a rule the value to modern medicine of these pioneer investigators was in an inverse ratio to the length of the period which separated them from modern times. Exceptions to this rule are found, however, even in the seventeenth and eighteenth centuries. Indeed, at that period when one considers the superstition, prejudice, mystic belief, magic, astrology, dogma after dogma, and system after system which prevailed, the inheritance of the dark ages, our admiration is excited by the really great results of the work of some of the scientists. Until the seventeenth century, Hippocrates, Galen, and Aristotle were the authorities in medicine. There was practically no advancement in medicine in that period of time. Anatomy and pathology were not understood; dissection was forbidden by the clergy of the Middle Ages, because it

was considered impious to mutilate a form made in the image of God. Dissections of the human body were practiced to a limited degree during the fourteenth and fifteenth centuries, but the sixteenth century was marked by the birth of Vesalius, a naturalist, whose investigations in human anatomy marked the beginning of scientific medicine.

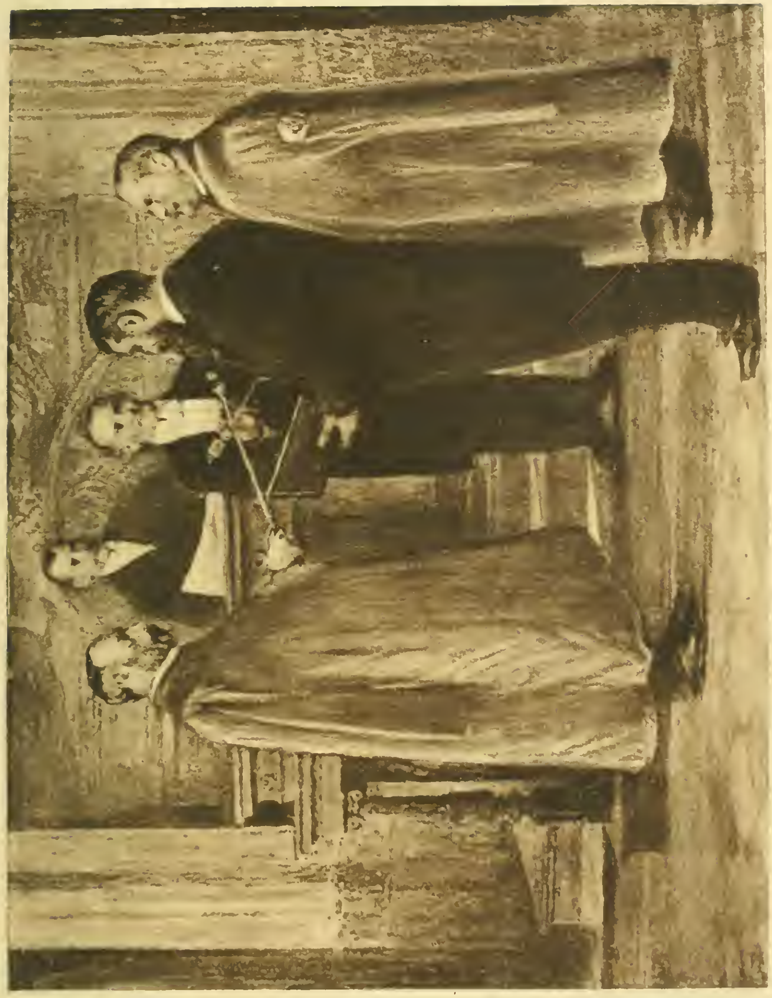
The seventeenth century marked the birth of realism. Galileo was a reformer in physics, and other scientific men broke away from the superstitions and dogmas of the day and searched for light along self-chosen paths. During the century, Harvey discovered the circulation of the blood. Zoölogy and botany were cultivated. Romer calculated the velocity of light. Lord Bacon's brilliant mind shone resplendent. Sir Isaac Newton discovered the law of gravity. Malpighi, Steno, Bartholin, De Graf, Wharton, Nuck, Brunner, Wirsung, Peyer, Havers, Cowper, Schneider, Hewson, Vieussens, and Merkel, and many others, dissected out everlasting monuments of their genius and skill. Hooke introduced the term "cell," and the cell-doctrine was founded by Malpighi and Grew. Linnaeus, Kant, Richelieu, Mazarin, Molière, Bach, Hayden, Beethoven, and Goethe were contemporaries of these other great men. Peruvian bark was introduced into Spain during this period.

The eighteenth century, called the golden age of medicine, witnessed a continuation of the constructive and realistic work of the previous century. Pathologic anatomy was born, and in the person of Morgagni received an impetus which gave it everlasting life. John Hunter, Baillie and Home in England, and Bichat in France were worthy successors of Morgagni. In this century Leopold Avenbrugger, the discoverer of percussion as a means of diagnosis of the diseases of large organs of the body, introduced the method in clinical investigation. Haller originated experimental physiology. An ambulatory clinic was

TAKING THE DOCTOR'S DEGREE.

Photogravure from the Painting by K. Storch.

The painter has depicted a typical scene in the life of a German University scholar. It is the memorable occasion when, some time subsequent to his graduation, he has written a successful thesis, and is having the customary oath administered after the University officials have conferred on him the coveted degree of a doctor of law.



inaugurated at Prague in 1745, and the first clinical institute was founded at Vienna in 1754 by Van Swieten. Preventive inoculation against small-pox was performed, a method of protection against variola which was practiced by the Chinese a thousand years before Christ. The most notable event of that period occurred at the close of the century with the discovery, by Edward Jenner, of vaccination as a protection against small-pox.

The period marked by the first seventy-five years of the nineteenth century was but a continuation of the tendencies of the preceding period. The watchword of medicine was pathological anatomy and diagnosis—the so-called scientific or exact medicine. This tendency to realism was modified to some degree by the philosophic teaching of Schelling, Hartman, Spencer, Haeckel, Hagel, and others. Pathologic anatomy found brilliant exponents in Bretonneau, Corvisart, Bright, Rokitansky, Louis Magendie, and many others. The practical salutary effect of pathology upon practical medicine was evinced by the epoch-making clinical observations of Addison, Graves, Cheyne, William Stokes, Trousseau, Wunderlich, Ziemmsen, Corrigan, and others. Notable was the advancement made in physical exploration in diagnosis. Avenbrugger's invention of percussion was extended by the translation of his book and the adoption and improvement of the method of percussion by Corvisart.

In 1815, Laennec invented the stethoscope. Skoda developed both percussion and auscultation and published his famous work on these subjects in 1839. Thus in medicine we find that, even in that early day, the pathologist and the clinician taught that by the aid of its special senses and by the microscope and instruments of precision the diagnosis could be made with a definiteness impossible by the use of the symptoms alone.

The epoch-making work of Johannes Mueller in em-

bryology and physiology marked the beginning of modern physiology, and this, with the unparalleled activity of Virchow in pathology, resulted in an enormous development of scientific observation and productiveness.

Corresponding activity marked the work in the sciences of chemistry, zoölogy, comparative and human anatomy, physics, botany, and general biology. The development of the microscope gave impetus to the study of the lower forms of life. In 1838, Ehrenberg regarded infusoria as animals. In 1852, Perty claimed that most infusoria should be assigned to the vegetable world. Cohn proved the correctness of this conclusion and perfected a classification. In 1837, Bassi discovered the parasitic nature of silk-worm disease. The parasitic form of favus and thrush was proved by Schoenlein and Nagel respectively. Dovaine recognized the anthrax bacillus in 1850. In 1857, Pasteur demonstrated that fermentation and putrefaction were caused by lower organisms and at the same time forever set at rest the superstition of spontaneous generation. Obermayer recognized the spirillum of relapsing fever in 1873. Bacteriology became an exact science with the discovery by Robert Koch of cultural methods which made the differentiation of germs possible.

The causative relations of bacteria and microörganisms to all infective processes has been proved by the laws promulgated by Koch. The discovery by Brieger, Panum and others of the poisons produced by bacteria was another important step in the progress of bacteriology as related to medicine.

From the discovery and development of bacteriology, and especially through the brilliant researches of Pasteur and Koch and of their students, has resulted a knowledge which has revolutionized and marked the birth of modern medicine.

Parasites

The discovery of the hematazoön of malaria by Laveran; the recognition of the ameba of dysentery by Loesch; of the ray fungi and especially the actinomyces as infective agents in the lower animals and in man, and the more exact knowledge of other animal parasites infecting man and animals, which the microscope has made clear, have been as epoch-making in parasitology as the discoveries of Pasteur and Koch in bacteriology.

The recognition of the relation of bacteria, protozoa, and animal parasites to infective disease has been the means of a more exact knowledge of the clinical phenomena of disease, of morbid anatomy, of physiology, and of physiologic chemistry than would have been possible without it.

Transmission of Infection

The knowledge of the cause of disease has led to a study of the life-history of infective organisms outside of as well as in the animal body. The mode of propagation, the means of transmission of infective microörganism, by fomites and other agents, has become known. The rôle of insects which infect animals play, as definitive or intermediate hosts, has been studied and proved. The discovery of Manson of the transmission of Filaria sanguinis hominis by the mosquito was of vast importance as a suggestion of the mosquito as a definitive host in malaria. The investigations of Manson, Ross, Celli, Grassi, Dionise, Marchiafava, Bignami, Koch, and others have made our knowledge of malaria exact. With the microscope we may now not only recognize malaria and differentiate it from the other infective fevers, but we may also at the same time recognize by an examination of the blood the type of malarial infection and foretell its course. Not only may we recognize

the disease definitely and apply the drug treatment more rationally, but the knowledge of the means of its transmission from man to man enables us to apply preventive measures which are of the greatest importance from a commercial as well as from a humanitarian point of view. The recognition of the rôle of the mosquito in malaria has been, furthermore, a stimulus to the study of the same insect in relation to other infections.

The brilliant research work of Reed and Carroll in 1900 in Cuba, by which they proved that the mosquito of the genus stegomya is the sole means of the transmission of yellow fever from man to man, is of great importance as a scientific fact. The influence of this discovery upon mankind, as a prophylactic against a disease which has killed multitudes, is wonderful.

Hardly less important is the fact that the Bacillus pestis may infect fleas and these in turn infect rats, mice, and man. It is important, too, to know that pests like the housefly may be carriers of infective bacteria from refuse filth to kitchens and tables and contaminate food, and thus infect us with typhoid fever, cholera, and perhaps other diseases which are propagated by filth.

The study of bacteria in the laboratory and in the blood tissues of infected animals has led to the discovery of the means by which bacteria disturb the animal economy and produce phenomena expressive of disease. The fact that the blood and tissues of infected animals contained a toxin which could also be isolated from pure bacterial cultures in the laboratory and that this toxin when introduced into an animal was capable of exciting the same phenomena of disease as the bacteria themselves, was positive proof that bacteria excite disease phenomena at least in some instances by means of a toxin which they form. The elaboration of antitoxins in the body of the infected animal was also

promptly recognized, and served to explain not only the self-limitation of many of the infective diseases, but it also helped us to understand the immunity which one attack affords in some of the bacterial diseases.

Protective Inoculation

Long before bacterial toxins were recognized as the cause of disease phenomena, Pasteur established the principle of protective inoculation with bacteria of lessened virulence, which was brought about by attenuation of the bacteria by a modification of cultural methods and also by serial inoculation of certain lower animals. This he successfully applied to charbon in sheep and cattle and to chicken cholera. In both of these diseases the bacteria were known and the problems of attenuation could be carried on in the laboratory by direct study of the bacteria before inoculation and afterward when they were recovered from the body of the animals experimented upon.

His final life-work was no less important in firmly fixing the immunizing influence in rabies. Here the discovery was made that the infecting bacterium escaped every known means of recognition by microscopical and cultural examination of the tissues and blood of the infected animals. Apparently there are pathogenic germs which we do not know because we have not yet recognized the proper culture material for the successful artificial cultivation of them, nor have we discovered the tinctorial reaction which they may possess; and, finally, it is not improbable that they may be infinitely smaller than other germs and, therefore, more difficult to recognize.

Pasteur recognized the fact that in hydrophobia the brain and other nervous tissues of an infected animal are capable, when inoculated into another animal's brain, of producing the disease. That the infected brain used for in-

fecting animals contained the germs which caused the disease was proved by the fact that a stage of incubation occurred in the inoculated animal and that a series of animals were successfully inoculated consecutively from the first. Pasteur then successfully attenuated the unknown microorganism present in the nervous tissues of an inoculated animal by dessication of the nervous tissue in a sterile apparatus by methods too well known to repeat. Nor is it necessary to occupy time in repeating the well-known methods pursued by Pasteur and his pupils in the use of the graduated doses of attenuated toxin contained in the nerve tissues in the prophylactic treatment of rabies. To Pasteur, therefore, we owe the scientific recognition of the principle of protective inoculation.

It is now a well-know fact, however, that inoculation against disease was practiced by the Chinese a thousand years ago. They inoculated the healthy with small-pox as a protection against the disease. Variolization was also practiced in Europe in the seventeenth and eighteenth centuries. We read that in 1718, Lady Montague caused a son to be inoculated with variola in Italy, and that two years later her daughter was inoculated in England. The practice was followed in Ireland long after the successful establishment of vaccine as a protection against variola. Inoculation against syphilis, or syphilization, was practiced in Europe during the nineteenth century.

We owe to Jenner, however, the first example of the protective inoculation by means of an attenuated virus. This attenuation we now know was established by the accidental inoculation of milch cows with small-pox, producing a modified disease, vaccinia. That vaccinia, produced in man by inoculation, would protect against small-pox was proved when, in 1798, Jenner successfully vaccinated direct from the cow, the five-year-old lad William Summers.

The thousands of successful vaccinations which have since been performed and the thousands of lives which have been saved by vaccination are proof of its validity and utility. The immunity established by protective inoculation is apparently the same as that induced by an unmodified attack of variola.

Serum Therapy

When chemistry had revealed the nature of bacterial poisons and experiments established their relation to the phenomena of disease, it was proved that substances were formed in artificial culture media and in the blood and tissues of infected animals which had the power to neutralize the effect of the bacterial poison in other animals infected with the same organism. Further investigation showed that an animal inoculated with the laboratory preparation of antitoxin was protected against the disease.

Furthermore, it was found that the blood serum of an animal inoculated with bacteria in a non-fatal and repeated dose contained an antitoxin. When the blood serum of an infected animal was injected into a healthy animal, the latter was protected against the original disease.

Antitoxin was, therefore, proved to be formed in artificial media of bacterial cultures and in the bodies of infected animals. When the antitoxin thus formed was injected into an animal, it had the power to protect that animal against the particular bacterial infection, or, if given subsequent to the infection of the animal, to mitigate the severity of the disease or entirely to check it.

Thus Koch and his students established the principle of serum therapy. Upon this principle there has been developed and given to the world the anti-diphtheritic serum of Behring and of Roux, and also an immunizing serum for Asiatic cholera, tetanus, erysipelas, plague, epidemic dysen-

tery, streptococcus infection, and other diseases. While the serum treatment has not proved successful in all of the diseases in which it has been used, it has been so successful in some—diphtheria, for instance—as firmly to establish the principle of serum therapy. The study of prophylactic sera by Paul Erlich led to our present knowledge of immunity. His side chain theory has established a working basis which affords superb fields of research in physiologic chemistry which have already yielded rich returns.

Bacteriology made possible the comprehension of perfect cleanliness and enables the surgeon to invade every part of the body without fear of infection and has saved thousands of lives which twenty-five years ago would have perished miserably as the result of disease at that time inoperable, or as the result of infection from contact with the surgeon. By means of cleanliness and skill, induced by a broader experience, the surgeon has been able to add to our knowledge information of great value which could have been obtained probably in no other way. He has been able to study disease in the living body and show the relation of a disease process to infection. He has thus been able to clear away many of the misconceptions of symptomatology and diagnosis, especially in disease of the abdominal organs.

Bacteriology has stimulated laboratory clinical diagnosis. Bacterial reaction to sera and blood cultural tests are of the greatest aid to diagnosis. Clinical research work has command of an armamentarium consisting of a knowledge of pathologic anatomy, of physiology, of bacteriology, of chemic physiology, and of physics, which allows of a precision in diagnosis never before at the command of the physician.

The evolution of bacteriology has afforded a stimulus and aid in the advancement of parasitology, physiology,

physio-chemistry, and of other fundamental sciences. This knowledge has been more directly applied to practical medicine than ever before.

Indeed modern medicine is now so comprehensive that the student must be thoroughly conversant with chemistry, inorganic, organic, and physical, with physiology, with general biology, with human and comparative anatomy, with bacteriology, and parasitology, to understand and appreciate it.

Slowly but surely the secrets of the cause of disease which baffled the search of centuries have yielded to the brilliant light of modern methods. The causative agents of most of the infective diseases of man and of the lower animals are now known.

The unknown causative germs of the few remaining infectious diseases will soon be discovered, and then the principles of immunity and cure by inoculation or by the application of antitoxins will find wider application.

Prevention of Infection

The recognition of the germ-cause of the infectious diseases enables modern medicine not only to combat disease more rationally and successfully, but it enables us to prevent them.

In most of the infective diseases due to germs, protozoa, parasites, and fungi, the causative agents have been so fully investigated that we know the life-history, and what conditions are best suited for the propagation and multiplication of each, and also what will remove and annihilate these dangerous enemies. So the diseases of domestic animals which may also infest man, for example, actinomycosis of cattle, trichina of swine, tuberculosis of animals, chicken cholera, foot and mouth disease, charbon, etc., may be entirely eradicated. The experience of one hundred

years proves that small-pox may be prevented by proper vaccination. If universally applied and repeated at proper intervals the disease would probably disappear.

Our knowledge of the living agents which provoke malaria, typhoid fever, cholera, the plague, and the means by which they propagate, develop, and the manner in which they infest man, enables us, if we may command the situation irrespective of the financial cost, not only to prevent but also in many localities to abolish them altogether.

The discoveries of Reed, Carroll, and Agramonti of the relation of the mosquito (Stegomyia fasciata) to yellow fever has been practically applied with notable success in Cuba and elsewhere.

The study of bacteriology has developed general hygiene to a high plane. The value of sunlight, pure air, and pure food are fully recognized as preventives and also as rational curative measures in many infective diseases.

Unfortunately there are a few of the scourges of mankind which science has not yet conquered. Pneumonia, the bacterial cause of which is known, is still a "captain of death." Cancer remains unconquered. So, too, do many of the chronic diseases, namely, the primary blood diseases, diabetes, the various degenerative processes, etc., which, though frequently easily recognized during life, are at best only modified by our efforts to check or remove them.

Physio-chemistry, experimental medicine, physiology, and pathologic anatomy have given us much information of these processes, and there can be no question that many of these problems will be solved by the present methods of investigation.

The present knowledge of the cause of disease, of the evolution of disease processes, of the natural expression of disease as recognized by clinical investigation, has resulted in a rational mode of treatment. Drug treatment

is no longer looked upon as specific, but as a helpful agent to modify and palliate disease processes, in conjunction with proper dietary, hydratic, and hygienic measures. Polypharmacy and indiscriminate drugging and drug nihilism are recognized as equally irrational. It requires a nice judgment of when to give, as much as when to withhold, drugs.

To enable a diseased or crippled organ more nearly to perform its function; to fortify and prolong life, with the hope of a favorable termination of a self-limited disease; to palliate suffering, are some of the measures which drugs afford modern medicine. Pharmacology and pharmacy have developed equally with the other parts of medicine and enable us to command drugs and active principles with accuracy and comfort.

The discovery of the X-ray was a boon to surgical diagnosis and it has proved of wonderful therapeutic value in many of the disease processes of the skin and superficial tissues. When the X-ray shall be better understood its appreciation will be undoubtedly much more extensive.

The rapid development of modern medicine has attracted wide attention and excited the interest of students and investigators over the whole world.

A larger percentage than ever before of the best-educated students of the world have sought medicine as the most attractive field of study and research. At this time there are hundreds of earnest, thoughtful, patient, and energetic workers after truth who frequently sacrifice home, friends, comfort, health, and even life for the advancement of the science of medicine.

The advancement of modern medicine has also attracted the attention of the philanthropic rich as never before. In recent years institutes of research have been erected or are in the course of construction and equipment which have

rich endowment. Modern medicine is therefore better prepared to develop now than ever before.

The development of medical literature has been in keeping with the advancement of other sciences. Large and valuable libraries are found in every land. Medical journalism is a science of itself and enables the physician at small cost to be in touch with all that is new and progressive.

Modern medicine requires of its students an education which shall fit them to take part as research workers or as practitioners to apply the measures afforded them to prevent or more quickly to modify disease. The modern medical student, therefore, requires the broad education of the university and a training of his special senses in the study of the natural and of the fundamental medical sciences, preliminary to the study of applied medicine and surgery. Happily both the old and the new world afford institutions which satisfy all requirements of modern medical education. Many medical institutions exist which cannot furnish the necessary educational advantages. These institutions are doomed. They are relics of the past. It is to be hoped that they will be no exception to the rule of the survival of the fittest.

THE RELATION OF PATHOLOGY TO OTHER SCIENCES

BY JOHANNES ORTH

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WHOEVER has to speak of pathology in general, as is my task, must first determine what he includes in pathology, for the ideas which are evoked by this term are not always the same. The opinion is common that pathology is synonymous with "science of disease," "nosology;" but this, as Rudolph Virchow[1] has attempted to prove repeatedly, is not true. Doubtless disease, or rather the diseased individual, is the most important object of consideration of pathology; it is, however, not the only one. The conception of pathology is much more comprehensive. To pathology belongs, on the one hand, every deviation from the normal structure and the normal composition of the body, and, on the other, every deviation from the normal function of its parts. It therefore includes every variation from what we consider the type of an organism. Variation from type is, however, not disease. Disease, is as Boerhaave was the first to say, *"Vita praeter naturam,"* and life presupposes activity. When there is no functional activity and thus no deviation from normal function, there can be no disease.

[1] *Handb. d. spec. Pathol. u. Therapie,* 1854, pp. 6 ff.

But not even every functional variation from the normal indicates disease. The variation must be pernicious in character, if it is to bear the name of disease. When there is no detriment, there is no disease, although whenever a variation from the normal exists, we have to do with a pathologic condition, no matter whether the variation is morphologic or functional.

Purely morphologic variations without detrimental influence on the rest of the body are found, especially among anomalies and malformations, and who will deny that these belong to the realm of pathology? An individual with a supernumerary nipple, a person with polydactilism, a woman with uterus septus or bicornis, all are pathologic, although none are sick. Thus, while the biologic phenomena of the diseased state form the greater part of the realm of pathology, they do not complete it. Its limits must be extended much further, but how far is the point of contention.

Many may consider the statement of Virchow[1] a witty paradox when he says that the development of new species really belongs to the realm of pathology, as a new species must find its origin in a variation or deviation from the preceding type, and variation from type is pathologic. Thus the whole teaching of evolution, the science of phylogeny, is to be considered part of pathology. I share throughout Virchow's opinion, and in my work on inherited and congenital diseases, recently published,[2] I have again given this fact expression that we must presuppose a variability of the embryonal protoplasm (*Keimplasma*) and that variation or deviation from the previous type either acquired or inherited or even arising from external influences is the necessary preliminary to the formation of a new species, sub-species, or variety. I would not, however, like

[1] R. Virchow, *Rassenbildung u. Erblichkeit, in Festschrift für Batian,* 1896.

[2] Orth. *Angeborene u. ererbte Krankheiten u. Krankheitsanlagen, in Krankheit und Ehe,* herausgegeben von Senator u. Kaminer, München, 1904, p. 26.

to go so far as to call everything arising in this way pathologic, no more than I can consider it pathologic when, by immunization, a man is made better than he was before. Such a man varies from the type of normal man, but is not pathologic, because the variation is useful and appropriate. Only variation which is inappropriate or useless is pathologic. I realize that it may often be difficult to determine the limits of the inappropriate and useless and thereby pathologic, especially in the development of varieties and races. Thus, I should not hesitate to class the Crested Polish fowl with its exencephalocele as pathologic, while I should exclude those breeds which the animal breeders have made for useful purposes from pathology, no matter how near the pathologic the products of skill might be.

Variations from type occur in inanimate as well as animate nature; there are malformed crystals just as there are malformed plants, animals, and persons, but we are not accustomed to speak of a pathology of crystals or stones, but only of plant, animal, and human pathology, for only with living beings can we rightly speak of useless, inappropriate, or pernicious variations from the normal.

Human pathology, undoubtedly the most momentous and important for us, has made but little use of plant pathology as yet, although there can be no doubt that many conclusions for general pathology as for general anatomy are to be drawn from botany. The reaction of plant cells to unusual conditions, and the morphologic and functional disturbances which occur under such circumstances are easier to observe, and may well serve as guides to the understanding of similar processes in animal or human cells. Experimental pathology has already made use of plants in its investigations,[1] but only recently have we begun to give more attention to

[1] O. Israel, *Biolog. Studien mit Rücksicht auf d. Pathol. Virchow's Arch.* 141, p. 209, 1895.

the spontaneous diseases of plants, especially since we have learned how great a rôle parasitism plays in vegetable as well as human pathology. At the head of the parasitic problems of human pathology of the present day stands that of the etiology of tumors; here cancer cells, here cancer parasites, so sound the battle-cries, and a parasitic new formation in the vegetable kingdom, the club-root of turnip, did not only have to furnish the paradigma of cancers in man and beast, but some investigators have even gone a step farther and see in *Plasmodiaphora brassicae,* the parasite of club-root, the exciting cause of animal tumors or at least a close relation of such cause.[1]

Very different is the relation of human to animal pathology, not only on account of the closer relation between man and animal, by reason of which a comparison of observations between animals, especially the higher vertebrates, and human pathology is more permissible, but also because the questions to be decided experimentally must be proved in the main on animals.

Even though a complete agreement between the phenomena of human and animal pathology cannot exist, as the function and construction of the animal body and its organs do not entirely agree with those of man; even though many diseases which attack man do not occur in animals, still analogies are not wanting and the similarity is greater the higher the group among the vertebrates to which the animal in question belongs. An especial advantage of comparative animal pathology is that the necessary material is not only easier to obtain than the human, but that particularly by voluntary killing of pathologic animals accurate morphologic investigations can be made at any desired stage and on perfectly fresh tissues free from cadaveric changes. Especially valuable conclusions can be drawn in those dis-

[1] Gaylord, *Zeitschr. f. Krebsforschung,* I, 1903.

eases, which are common to man and animals, the zoönoses and the anomalies of formation, the simpler ones as well as the monsters in the narrower sense.

A somewhat neglected realm of comparative pathology has recently attracted the attention of pathologists in more and more increasing degree; namely, tumor formation in the lower animals.[1] From their construction we may expect to draw valuable conclusions in regard to the pathology of human tumors, not only in the morphologic but also in the genetic direction. One point especially comes into consideration, which also plays an important part in the utilization of animal pathology in other directions, the possibility of purposeful inoculation experiments from animal to animal.[2]

Unfortunately the great value of experimental research for all branches of pathology[3] is not sufficiently known among the laity, and attempts through governmental interference to lay difficulties in the way of experimental investigation (vivisection as it is called by the laity, scientific animal torture according to its opponents), are constantly being made, not seeing that misuse of it, even if it should occur, is considerably outweighed by its undeniable value. Pathologic anatomy, bacteriology, pathologic chemistry, and above all, pathologic physiology, cannot fulfill their scientific value without animal experiment. A large part of the progress in pathology is bound up with experimental research. Every advance in pathology has sooner or later been of use to man. Could our progress in the pathology of the infectious diseases, and our progress in the prevention and treatment of them, have been made without ex-

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[1] Pick u. Poll, Berlin, Klin. Wochnsechr, 1903, p. 518.

[2] C. O. Fenger, *Experim. Untersuch, über Krebs bei Münsen, Abh. f. Bakterio*, xxxiv, p. 28, 1903; Borrel, *Epithél. infecticuses Ann. de l'Inst. Pasteur*, 1903, no. 2.

[3] R. Virchow, *Ueber den Werth des pathologischen Experiments*, Internat. Med. Congress, London, 1887, Berlin, 1899.

perimental pathology? The explanation of the origin of tumors must also finally arrive by experimental investigations, and just there it will be of especial value to be able to carry on the experiments on the same kind of animal in which the tumor naturally occurs. If we should succeed in finding a specific, probably parasitic cause, the possibility of demonstrating the pathogenicity of this disease-producer on animals of the same sort is incalculable. But such experiments presuppose exact knowledge of the pathology of the animals experimented upon, that is, comparative pathology, and many discussions of the present day have turned on the point whether changes which were found after the experiment were results of the experiment or chance pathologic findings to which the experiment had no genetic relation. If one does not know what kind of tumors occur in the organs of the animal which he is using for experimental purposes, he will easily fall into the danger of considering new formations as the result of the microörganisms injected by him and will report having produced a tumor when merely a spontaneous new growth existed.

So far I have considered animals only as passive objects of experimental pathology. I have spoken of animals and plants merely as the most important subjects for comparative pathology. There are, however, much closer relations between pathology and botany and zoölogy. Both these sciences have had increasing importance for pathology, as surer proof was brought that the most important causes of disease belong to the plant and animal kingdoms.

Investigation of the causes of disease, of the different conditions which form the basis of deviations from normal types, belongs as much in the realm of pathology as the study of these deviations and their development itself. The etiology and pathogenesis are a part of pathology, and it

is especially through them that pathology has its closest relationship with the other sciences. Mechanics, general and cosmic physics, geology not less than geography, inorganic as well as organic chemistry, social and military history, sociology, and commercial science, etc., must all be considered for the enlightenment of the etiology of disease and the explanation of the appearance of disease, especially in regard to time and place (historic geographic pathology). But above all stand zoölogy and botany, for the most important and most common diseases are produced by living beings, by parasites.

It is an old statement in pathology that a parasitic relation exists in disease. For a long time the disease as such was thus personified; it was spoken of as an organism within the organism, a parasite, which as Wunderlich[1] said, was anthroposed or phytomorphosed in every way. To it was ascribed an existence, a growth, limbs, and organs, a power of endeavor and of thought, even a sickness, death, and finally a corpse. Pathology has done away with this conception. It is true that we still speak of the disease, of cholera, typhoid fever, pneumonia, etc., and that in practical medicine we still speak of treating this or that disease. A treatment for syphilis, for diphtheria, or some other disease is recommended as if we spoke of something tangible, independent. But all this is only for convenience of expression, and we know very well that what we call a disease is not an entity but only a group of phenomena which have for their basis a common cause. There are really no diseases, but merely sick men, diseased organs, diseased tissues, diseased cells, and it is the cause of these disturbances which brings about the special phenomena which we observe in the diseased part.

This cause may be a parasite. Centuries ago the opinion

[1] Wunderlich, *Hdb. d. Patholog. u. Therap.* I, p. 12, 1852.

was occasionally expressed that diseases were caused by living beings, which disturbed the life-processes in the human body. In the middle of the last century the view that there must be *contagion vivum* was victoriously upheld by Henle,[1] but only in the last decades of the nineteenth century was actual proof brought forward that by far the commonest causes of disease are living organisms which live parasitically on or in the human body. The disease is not the parasite, but one parasite or many parasites cause those variations from the normal structure and function of parts of the body which in their entirety we call disease.

By parasitology a close union is made between pathology and the described natural sciences and thus with general biology.

The great biologic question as to the origin of the lowest being is related principally to the human parasites. In spite of the statement of the great English physician Harvey, "*Omne vivum ex ovo*," the doctrine of spontaneous generation, which ruled for thousands of years, had not vanished from science, and in the beginning of the last century natural philosophy treated with preference on the beginning of life, and some are not lacking in our day who believe that they see in the doctrine, that the tissues of our bodies break up in decomposing into small organisms,[2] an expression of the immortality of the life principle.

That the large intestinal worms do not arise from the dirt of the intestinal canal, from saburra, but that for them Harvey's rule holds, has been shown by both zoölogists and pathologists. For the smallest beings we may mention the chemist, L. Pasteur, with the physician, Robert Koch,

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[1] Henle, *Hdb. d. Ration. Pathol.* II, 2 p. 457, Brauschweig, 1853.

[2] R. Arndt, *Unters. über d. Entschung von Kokken und Bakterien in organischen Substanzen*, Virchow, *Arch.*, 82, p. 119, 1880; A. P. Tokker, *Versuch. einer nauen Bakterienlehre*, 1903.

the former of whom conclusively disproved the spontaneous generation of microörganisms; the latter as the discoverer of the methods which permitted us to ascertain simply and surely the constancy of form of a microörganism and to give incontrovertible proof that in every single microörganism the law of generation was true, not entirely in Harvey's sense, but in the more general form: *Omne vivum e vivo ejusdem generis.*

But it is not only general biology which has been furthered by the parasitology of the physician, but also special biology and the systematic classification of parasitic animals and plants. Just here is plainly shown that pathology cannot in any way be separated from the other natural sciences, as it is not only the receiver which makes practical use of scientific discoveries, but also the producer which by its own effort, and through independent performances furthers science. The modern development of bacteriology, the determination and elaboration of exact methods of investigation, the morphology and biology of bacteria, have not been entirely developed by botanists, but it has been and still is physicians and pathologists who may claim a large part of the results as due to their efforts.

The same relation in working together exists between pathology and zoölogy in regard to the parasitic animals. Here the points of contact of the two sciences are doubled, for on one hand the change of generations of many human parasites, their occurrence in different hosts, as well as the fact that animals may be the simple conveyers of parasites, required the human parasitologist to bring the animal world into the realm of their investigations; on the other hand, the morphology and systematic study of the parasitic animals themselves has been ascertained with considerable assistance from pathologists. In the first class I will only recall the joint work of pathologists

and zoölogists on trichinosis.[1] In determining the relation of this disease in pigs and other animals to that in man; malaria and the rôle which anopheles play therein; the recent investigations on the conveyance of plague and other infectious diseases by animals. Names of physicians like Küchenmeister,[2] Davaine,[3] and others have given human parasites their final place in zoölogy. I wish also to call attention to the very recent investigations concerning protozoa as disease-producers, one of the most burning questions of modern pathology, a question of extreme importance, and also of correspondingly great difficulty. Unfortunately, investigations on the parasitic protozoa remain still in their infancy, but even on this question the pathologists of Europe and North America may demand recognition of their zealous work.

Closest and most numerous are, of course, the relations of pathology to anatomy and physiology. Just as the study of the normal, typic man is divided into anatomy and pathology with physiologic chemistry, so also is pathology (apart from etiology and pathogenesis) made up of pathologic anatomy and pathologic physiology with pathologic chemistry. Just as health and disease pass imperceptibly into one another, so there can be no sharp line drawn between pathologic and normal anatomy, normal and pathologic physiology. These studies are not different sciences, but branches of the same scientific tree with the same stem, the same roots. Their methods of investigation are mainly the same. Discoveries in one generally mean progress in the others.

The time is not long past when instruction in pathologic anatomy in our universities was in the hands of the pro-

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[1] Zenker, *Arch.* I, p. 90; Leukart, *Unters. üb. Trich. spiral.* 1866, R. Virchow, *Lehre von den Trichinen.*

[2] Küchenmeister, *Die in und an d. Körper d. lebend. Menschen vorkommenden Parasiten,* 1878, 1879, 3. Aufl.

[3] Davaine, *Traité des Entozoaires,* Paris, 1877, 2 Aufl.

fessor of normal anatomy, and when men like Joh. Fr. Meckel, Johannes Müller, and others enriched and fostered normal as well as pathologic anatomy. Pathologic anatomy is only conceivable on a basis of normal anatomy, and a glance at the history of medicine shows how every progress in normal anatomy has produced an increase in the knowledge of pathologic anatomy. Only the flourishing of anatomy in the sixteenth century made the development of pathology to a separate science during the ensuing century possible. But here also pathology was not only the receiving but frequently the producing science. Pathologists not only enriched anatomic and histologic methods, but contributed largely to the development of accurate anatomy, the general as well as the special. Who does not think in connection with "general anatomy" of Rudolph Virchow,[1] the man who coined the famous words "*omnis cellula e cellula*" corresponding to Harvey's "*omne vivum ex ovo?*" That saying while resting in great part on pathologic observations, is equally true for pathologic and normal anatomy.

In connection with special anatomy it will suffice to refer to the progress in the anatomy of the brain, especially to the course of its fibers, in order to show how much pathology has contributed to the knowledge of normal structure. The great progress which the fine brain anatomy made in the last decades of the last century is due in large part to pathologic observations, medical investigations, methods conceived by physicians, and the result of investigations has been brought forward in connected form, especially by medical writers.

The same is true, but even to a higher degree, of physiology, the pathologic branch of which has unfortunately not

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[1] *Die Cellularpathologie in ihrer Begründung auf physiologische und pathologische Gewebelehre,* 1 Aufl. 1858; 4 Aufl. 1871.

received the deserved recognition and fostering in every place as a separate science, but which nevertheless has not been neglected by scientific medicine.

A large part of our knowledge of human physiology has been obtained by the observations of functions changed by disease as they appear as symptoms of disease in man or are produced artificially by experiment on animals. Where would the physiology of the brain be, if pathology had not made clear the position of the centres and the course of the tracts from the constantly recurring symptoms and lesions and pathologic experiment had not proved the correctness of the conclusions which were drawn from human observations?

What would general cellular physiology be, if observation of the behavior of cells under varying life conditions had not given us information concerning the processes under normal conditions?[1] Is not general cellular physiology rather a product of cellular pathology? Was it not a pathologist, R. Virchow, who introduced the idea that the cell is the final form element of all vital phenomena, and who arrived at this conclusion not least through pathologic observations?

From the deviations one recognizes most readily the law. There is no problem of general biology which has not received enlightenment and explanation from the experiences of pathology. The doctrine of heredity, to name only a few of these problems, plays no small rôle in pathology, and many cases of pathologic heredity throw a clear light on the subject and nature of heredity in general. The latest discoveries of pathology in the realm of hematology, the doctrine of agglutinins and precipitins, has already led to most valuable revelations respecting the general biologic question of the blood relationship of animals with

[1] Verworn, *Allgemeine Physiologie*.

one another, and of animals with man. The blood of anthropoid apes and man shows similar behaviors, but differs from the blood of other animals.

Especially numerous and close relations exist between pathology and that branch of biology which treats of the development of the human and animal body, and these relations are daily becoming closer and more numerous, as more and more frequently it can be proved or at least made probable, that pathologic phenomena, of all kinds form the basis of ontogenetic disturbances of the greatest variation.

An important difference exists between normal and pathologic anatomy, in so far as the genetic consideration plays a much greater rôle in the latter than in the former. Finished conditions form the basis of descriptive anatomy. Pathologic anatomy must always consider phases of development and none of its observations can be understood if their origin cannot be explained and if the original condition and the further development of its changes cannot be determined. The original condition, however, leads more and more frequently back to the time of embryonal development. It is to the eternal merit of Joh. Fr. Meckel,[1] the anatomist and pathologist, of Halle, that he showed for the first time in the case of a malformation of the intestinal diverticulum that the essential part of the variation from the normal consists in this, that a condition which is normal for a certain period of embryonal life, but which should only have a transient existence, is retained and is always recognizable in later stages of development, even though changed by the progressive growth of the part. This demonstration was the more important and valuable, as it treated of a theme which had hitherto been the ground of the most remarkable genetic theories. The apparently planless variation from type was explained as the work

[1] J. F. Meckel, *Handb. d. pathol. Anat.* I, p. 553.

of demons or devils or as a freak of creative nature (*lusus naturae*). Now, it was shown for the first time that also in the realm of malformations, order and law governed the process and not arbitrariness and freakishness, and that we must consider the embryonal development of these malformations if we would understand and explain these methodic processes.

Thus was founded the doctrine of imperfect development and growth, and as the basis for the explanation of malformations (*Hemmungs-Missbildungen*) it has been especially fruitful, as the fissures about the face, malformations of the female genitals, and congenital malformations of the heart will show, but that they have not yet closed the list is shown by the recent investigations of cystic kidneys, which have proved these to be due to a checking of the development of the embryonal organs. These examples show that disturbances of embryonal development are not only of importance in causing variations from the type, such as malformations, but also for disease-processes in the narrower sense, which originate most readily in malformed parts or organs. The idea that congenital heart disease was due to endocarditis in fetal life was largely due to the knowledge of the susceptibility of the malformed part to secondary so-called chronic inflammation. This is true not only of the macroscopic conditions like those mentioned, but it also favors the idea that incompleteness in the formation and the later development of a part cause a local disposition to disease. But this is only one side of the relationship between disturbances of development and disease. Another, perhaps even more important, is that which treats of the development of tumors on a basis of disturbance of development. The tumors of undescended testicles, the origin of new formations from displaced adrenal fragments, are as familiar to pathology and as surely established as

the occurrence of dermoid cysts, which can only be explained on the basis of the history of development. The well-known theory, according to which all tumors depend on disturbances in embryonal development, still lacks sufficient proof. Both pathologists and embryologists have been successful in showing, however, that one tumor at least, the dermoid of the ovary, only finds a satisfactory explanation in the presence of derivatives of all three embryonal layers, thus indicating a very early disturbance of development.[1] These tumors are closely related to malformations and pass without sharp division into true monstrosities. The study of all malformations, not only those due to impeded development and which no one attempts to deprive pathology of, is not to be separated from the study of normal development, for the origin of malformations goes back to the earliest embryonal period, and not only malformations of the whole body but anomalies of its single parts can only be understood and their origin explained in the light of normal developmental processes.

On the other side, experimental teratology, which is doubtless a branch of pathology, has made most important advances in the knowledge of the laws of normal development, the laws which govern the details of the regular formation of the embryo. Here also no sharp line can be drawn between pathology and embryology. Pathology takes its place alongside of embryology, with equal right and equal importance.

Thus we see pathology placed centrally among the biologic sciences, bound inseparably to all of them, not subordinate to any but their equal, receiving help from all sides but giving as much in return. Lastly, it must be stated that it is the problem of life which forms the subject of

[1] Marchand, *Eulenburg's Real Encyclopaedia*, xv, 432, 1897; Bonnet, *Ergebn. d. Anat. u. Entwicklungsgesch.* IX, 820, 1899; Wilms, *Die Mischgeschwülste*, 1899-1902.

pathologic work. Even though it wanders in its own ways, and possesses its especial questions, it is finally led to the general question of every biologic investigation.

Points of contact with philosophy are always presented by these general biologic problems, and we need only name Lotze,[1] the physician and philosopher, and his work on *General Pathology as a Mechanical Science,* to find the close relationship between philosophy and pathology personified in modern times. Metaphysic consideration of empiric assertions is necessary, as Kant has taught, to draw general conclusions and formulate general rules and laws from the observation of nature. Biology, and not least, pathology, lead everywhere to the limits of our knowledge of nature, where fixed knowledge finds its end, where we must, with Du Bois Reymond,[2] acknowledge our ignorance of what lies beyond, but where philosophic contemplations point a higher and more general way out of our difficulty. These limits to our knowledge are not lasting, however, for pathology. We will not remain in ignorance as long as the knowledge of healthy and diseased life progresses, and the boundaries of natural science and philosophic contemplation of the problems are being extended. Increasing knowledge of facts must be the basis of philosophic contemplation, if this would have real value.

There was a time in pathology when philosophic conceptions outweighed all other considerations, and when it was believed that all the problems of general biology and those of general pathology could be solved by pure reasoning. This period of natural philosophy was as unfruitful for real progress in pathology as the period of dogmatism in the Middle Ages, when Aristotle and Galen were looked upon

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[1] Lotze, *Die allgemeine Pathologie und Therapie als mechanische Naturwissenschaft,* Leipzig, 1842.

[2] Du Bois Reymond, *Ueber die Grenzen des Naturerkennens,* Naturforscher-Versammlung in Leipzig, 1872.

as the sum of all wisdom, and pathology was nothing more than philology, as all scientific work consisted principally in criticising and commenting upon the Greek writings.

This changed only after we emancipated ourselves more and more from the old dogmatic belief and through original investigations laid a true scientific foundation for pathology. The maxim of the great Morgagni,[1] *Nulla autem est alia pro certo noscendi via, nisi quam plurimas et morborum et dissectionum historias, tum aliorum tum proprias collectas habere et inter se comparare,"* as well as his other, *"Non numerandae sed perpendendae sunt observationes,"* had to receive general recognition before pathology was enabled to take its place among the other natural sciences. This place it had lost, for in the renaissance of science in the sixteenth century pathology stood in close relation to the other natural sciences; and medicine was for centuries the bearer of all natural science and included all other sciences within itself, so that not only did the teachers of other sciences belong in many cases to the medical faculty, but zoölogy and botany, physics and chemistry, were taught by physicians. We need only recall Haller and his great teacher Boerhaave, who successively occupied the chairs of botany and chemistry, of practical and theoretic medicine, and attained fame in all these branches. All this has changed in the course of time; the children have separated from their mother and have further developed themselves, and their development to great sciences has proceeded more rapidly than that of pathology. The time is not long past when the emancipated looked down on pathology and would not recognize it as an equal science. Did not Virchow find it necessary, before the congress of German naturalists, in 1867,[2] to insist on the scientific equality of pathology, and

[1] *De sedibus et causis morborum, per anatomen indagatis*, 1761.

[2] *Ueber die neueren Fortschritte in der Pathologie*, Vortrag in der 2. allgemeinen Sitzung am 20. September, 1867.

to demand that the so-called exact natural sciences should recognize pathology as an equal companion.

In fact, as pathology (excepting in purely etiologic studies) cannot do without physics and chemistry, as she also strives to refer pathologic phenomena to physical and chemic laws, so she has given something to these sciences and even to the present time has furnished workers which have assured themselves a lasting place in the history of exact sciences. Is not the mention of the name of the physician, Robert Mayer, the discoverer of the law of conservation of energy, and of Helmholtz, who began his professorship in Königsberg with lectures on general pathology, sufficient proof? The literature of Röntgen, radium, and other light-rays shows sufficiently how to this day pathology takes part in the investigation of physical problems.

These investigations lead to another especially important field, that of chemistry. Questions which were determined in the chemical laboratory of my institute, the proof, namely, that by the effect of radium rays on cancer tissue impediments which stood in the way of the action of preëxisting cytolysins are set aside, are nothing but chemic questions. Thirteen years ago I stated in a rector's address,[1] that only pathologic chemistry on a basis of cellular pathology could take us further in the study of infectious diseases, that the chemistry of bacteria, the normal and pathologic chemistry of the cells, was the problem of the future. This statement can be enlarged upon; in whatever branches of modern pathology we seek to progress, we finally always meet chemic questions, and it needs no prophet to tell us that the greatest progress of pathology in the immediate future will be along the lines of chemistry. In all directions pathologists have united with chemists to further the study of the chemistry of proteids. Physi-

[1] *Ueber d. Fortschritte der Actiologie,* Göttingen, 4. June, 1891.

cians and pathologists have furthered the knowledge of precipitins, agglutinins, and lysins of various sorts, not only in their practical but also in their purely scientific relations, and have begun to study these substances along different lines.

Pathology stands in close relation not only with that group of physical sciences which treat of life-processes and living organisms but also with the exact physical sciences. To these also many bridges lead, over which the connecting links flow in both directions, pathology giving as well as receiving. A separation of pathology from the other sciences could therefore only be made by force, for pathology forms an integral part of the science of life, biology. I do not consider it just, therefore, that in this Congress, bacteriology, which draws its greatest importance from that part which belongs to pathology, which is thus, principally, a part of pathology, has been placed by itself in Division C, "Physical Sciences" (*Naturwissenschaft*), and pathology in Division E, "Useful or Utilitarian Sciences." Is bacteriology not an eminently useful science? Has it not found the most widespread use in medical practice? Have not other branches of pathology, and especially pathologic anatomy, been reproached because it has done little for the prevention and treatment of disease, while bacteriology has done much in this direction? Yet bacteriology is put under physical sciences and pathologic anatomy with the rest of pathology among the utilitarian sciences! On what grounds can we consider human pathology as a different sort of science from the pathology of plants? If we class plant pathology with plant morphology and physiology as a part of biology (as is right), one must do the same for human pathology and place the biologic sciences in the closest relation with human anatomy and physiology. Human pathology is as much natural science and a separate branch

of biology as is phytopathology, and pathology is no more a utilitarian science than normal anatomy and physiology. Is medical activity conceivable without anatomy and physiology? As little as without pathology! Has pathology only importance through its relation to practical medicine? Not at all. Pathologists also prosecute their scientific studies without regard as to whether their work will be of immediate practical value or not. They also follow the inner motive toward knowledge and truth. They wish to satisfy that desire for increased knowledge which is in every human breast, to share in disclosing the secrets of nature. If the acquisitions of pathology have had a greater and more immediate effect on medical treatment than those of anatomy and physiology, that does not alter its scientific quality in the least; that they were also useful has never injured other sciences or lessened their scientific value. No one will value chemic and physical sciences less because they have been the basis of the wonderful advance in technic and industry, as displayed to the wondering eyes in this exposition. Pathology rejoices in its relation to practical medicine and would neither miss nor lessen it, for as physics and chemistry constantly receive from practice stimulus to new endeavors and progress, so also pathology needs uninterrupted relation to medical art. But it remains first of all an independent physical science, which in its three branches, pathologic anatomy, physiology, and chemistry, stands on an equal plane with normal anatomy and physiology and physiologic chemistry, with them and etiology forming the scientific basis for practical medicine.

But as for ages past a certain socialistic or rather humanitarian spirit has ruled in medicine (and to medicine pathology must always belong), which effected that will all pride over scientific demonstrations the real and true joy over scientific progress was not reached, if not only wisdom

and knowledge were furthered, but also something of value has been accomplished for the general good, so it may also remain in the future. Pathology will be recognized as a natural science, but it will be its pride and joy also in the future to be and to remain a utilitarian science.

THE PROBLEMS OF THERAPEUTICS

BY SIR LAUDER BRUNTON.

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THE subject of my lecture to-day is "The Problems of Therapeutics." My audience is a select one of persons interested in science and art. But science in these days has branched out so widely that it is impossible for any single person to be acquainted with every department of it, so that the terms used by a zoölogist may be unintelligible to a mathematician, or *vice versa.* There are some here whose researches have led them far into abstruce departments of science and if they were speaking I should gladly welcome a few introductory words from them on the very rudiments of their science in order to help me to understand a disquisition on the more advanced parts of their subjects.

Judging others by myself, I think they may be glad if I do the same, and I must beg the indulgence of those acquainted with medical science and its branches if this lecture should seem to be unnecessarily rudimentary. By therapeutics we mean the methods of healing. In the great staircase of St. Bartholomew's Hospital in London there is a large picture by William Hogarth representing the Good Samaritan. The poor traveler is seated on the ground, the Good Samaritan is pouring oil and wine into his wounds, while close at hand is a dog busily engaged in licking a cut which he has received in the fray. Both dog and man are engaged in solving, as far as they can, two of the primary

problems of therapeutics, viz.: (1) how to relieve pain, and (2) how to restore health. For disease is want of ease, and health is only one form of the word "whole," by which we mean that a thing is entire and neither cut, broken, nor cracked. The closure of wounds is one form of restoring "wholeness" or "health" to the body, but it is by no means the only one, for the vital organs lie below the surface, and it is with disturbances of their functions, even more than with external wounds, that therapeutics, or the science and art of healing, is chiefly occupied. As exemplified in the dog or in the Good Samaritan, therapeutics is simply an art. Certain things are done because they have been found to do good before and so they are repeated again and again, but neither the dog nor the Good Samaritan understands the reason why their procedure is useful. It is only when we learn the reason why that an art becomes converted into a science. Therapeutics in its primitive form is one of the simplest of all the arts and is practiced by animals as well as by man, but as a science it is one of the most complex and most difficult of all because it requires a knowledge of the functions of the body in health, or physiology; of their changes in disease, or pathology; of the actions of drugs upon the body, or pharmacology; and of chemistry, physics, and other sciences on which physiology, pathology, and pharmacology are based. Finally it requires the practical power of recognizing from the symptoms (in any individual case) the nature of the pathological changes present and the ability to apply the right methods of treatment in order to counteract these changes and heal the patient. It is evident that such complex knowledge as this must be very difficult of attainment, yet nevertheles the change of therapeutics from an art into a science is progressing with considerable rapidity. In a text-book on the subject which I published eleven years ago, I mentioned the use of quinine in ague

as the best example of the art of therapeutics whereby we could cure a disease of which we did not know the nature by a remedy whose curative action we did not understand. Since that time, however, we have learned that ague depends upon the presence of a foreign organism in the body and that the benefits obtained from quinine are due to its poisonous action upon this intruder. This malarial parasite is only one of the many minute organisms which mar or destroy the health of the human body. Minute organisms or microbes are most useful in their proper place and without them the world would be uninhabitable because they are the natural scavengers which produce putrefaction in dead plants and animals and thus bring about their return to dust, fitting them for new life instead of allowing them to incumber the ground. But not content with this function some of them proceed to invade living beings, attacking not only the weak but even the strong, and by growing and multiplying within them weaken or destroy their hosts.

One of the great problems of therapeutics, then, is to defend the body from attacks of microbes. This may be done either (*a*) by weakening or destroying the microbes themselves or (*b*) by increasing the power of the organism to resist them.

It is convenient to speak of the body as a whole when we are discussing its invasion by microbes, but we must not forget that the body, like a country, is composed of many parts. The interests of the different parts are by no means identical, and while they generally act together for the common good they may not always do so, and either by their sluggishness and inaction or by their mischievous activity may do harm instead of good to the body as a whole. What is requisite for health is an harmonious action of all the different parts of the body, or as St. Paul very well puts it, "And thus all the body framed and knit together through

that which every joint supplieth, according to the working in due measure of each several part, maketh the increase of the body unto the building up of itself" (Ephes. IV, 16, Revised Version), so "that there should be no schism in the body and that the members should have the same care one for another." No doubt in their long wanderings together Luke the beloved physician discussed physiology largely to Paul, and his expression is so good that I introduce it now.

Just as the people of a country is composed of individuals, so the body is composed of numerous cells. The whole class of microbes consists of isolated cells which are like a nomad population, each individual complete in himself, and all ready to form a swarm for attack and invasion. The cells which compose the body, on the contrary, are mostly fixed, and differ from each other in structure and function, but ought all to act together for the common good, like civilized people. Each cell lives in the fluid which surrounds it, blood or tissue juice, from which it takes what it needs for its own nutriment and pours back the products of its tissue activity which may be partly waste and partly manufactured products of the utmost utility.

In order to have a complete comprehension of therapeutic problems it is necessary that we should know something about the life of the cell, because the life of the whole body depends upon that of the cells which compose it, and the cure of disease and the preservation of life depend on our power to influence cell-life. The processes of life are to a certain extent the same in the human body as a whole, in the cells which compose it, and in the smallest living organisms or microbes as they are termed. They all digest and assimilate food, they all breathe, and they all excrete waste products. A knowledge of the proceses of life in man helps us to understand them in low organisms and *vice versa*. The use of pepsin and pancreatin in indigestion is so com-

mon that almost everybody knows that these substances have the power of dissolving meat and that pancreatin converts starch into sugar. Everybody knows that these are got from the stomach and pancreas of animals and that it is by similar substances formed in our own digestive canal that we are able to dissolve the food we eat and render it fit for absorption. It has recently been found that pancreatic juice, as poured out by the gland which secretes it, is very slightly active, but it is made active by another ferment secreted from the intestine which is called enterokinase. The pancreatic juice contains several ferments; that which acts upon meat is called trypsine and in its inactive state it is called trypsogen. The action of the enterokinase on the trypsogen may be compared to that of a man who opens the blade of a knife and renders an instrument previously inactive very active indeed. If trypsine were absorbed into the blood unchanged it might digest the tissues themselves and it must be rendered again inactive. This seems to be effected by certain substances present in the blood which have a so-called "anti" action upon the ferments and render them again inactive. But though the digestive ferments might do harm if present in the blood in an active form and in large quantity, yet it is probable that all the cells of the body digest the food which is brought to them by the blood and tissue juices and break up this food for their own use by ferments which they contain themselves. Thirty years ago I advanced this view and supported it by the fact that I was able to extract from muscle by glycerine a substance which decomposed sugar. This observation received but very little attention at the time, but recently German literature is full of papers which support my views and confirm my results, although their writers apparently are ignorant of my work. Fifteen years ago, along with Dr. Macfadyen, I showed that bacteria not only excrete ferments by which

the soil in which they are growing is digested, but that they are able to modify these ferments in accordance with the soil so as to digest either proteid matter or sugar. Curiously enough, within the last few years the pancreas in animals has been shown by Professor Pawlow to have similar powers.

No individual microbe has received so much attention as the yeast plant and no poison which is formed by any of them has done so much harm as the toxin or poisonous substance produced by yeast, for this toxin is alcohol, whose poisonous action has given rise to the term intoxication. The yeast-plant, when grown in sugar, excretes into it a ferment, invertase, which splits up ordinary cane-sugar or saccharose into two other sugars, dextrose and levulose. The yeast-plant may be separated from the solution of sugar by filtration, but the ferment which is already excreted will remain in the filtrate and may still continue to act on the sugar, just as pepsin may dissolve a piece of meat in a jar although the pig which produced it is dead and gone. But no alcohol will be formed by this excreted ferment. Alcohol is produced by something contained within the body of the yeast itself and its production was formerly supposed to be due to so-called vital action. It has now, I think, been proved that alcohol is produced by the action of a ferment which is contained within the body of the yeast-cell and is not excreted from it, so long as the cell is intact, but only passes out after the cells have been crushed into fragments. Whilst the cell is alive and intact it absorbs the sugar into its interior, breaks it up there, and forms the alcohol which is afterward excreted.

To make this clearer I may perhaps be allowed to use a very crude illustration and compare the ferment which is excreted by a bacillus or by yeast to the saliva which is said to be poured out by a boa-constrictor over its victim to fa-

cilitate its ingestion, while the ferments within the microbe may be likened to those in the stomach and intestine of the boa by which it effects the digestion of its prey.

Other microbes in like manner absorb nutriment and may form and excrete toxins, though both the nutriment and the toxins of bacilli in general differ from those of yeast.

To recapitulate what I have already said, we see therefore that

(1) Cells excrete ferments;

(2) They excrete poisons formed within their bodies; and

(3) When they are broken up they may liberate other ferments.

The ferments excreted by microbes apparently prepare the substance in or on which they are growing for assimilation, and the ferments within the cell-body decompose it further in the process of growth. It is probable that all cells, whether they be wandering microbes or cells coördinated in an organism, prepare and assimilate their nutriment by means of ferments, and Macfadyen and I found that not only have bacilli the power of excreting ferments, but apparently they are able to adapt the ferment which they excrete to the soil in which they are growing in much the same way as Pawlow has recently shown that the pancreas in animals modifies the ferments it forms according to the food which it is required to digest.

Not only is digestion carried on in the stomach and intestines by the ferments which are now so well known even to the general public, pepsin, pancreatin, etc., which dissolve the ingested food so that it is readily absorbed into the circulation and carried to every part of the body, but the other cells which compose the various parts of the body, muscles, nerves, and glands, probably carry on the functions of their life by means of ferments also. By means of these they

alter and assimilate the various substances which are brought to them by the blood and juices of the body, and after having supplied their own wants they throw into the circulation the altered residue of their pabulum as well as the substances which they have themselves formed in their processes of growth. They probably repeat in fact what we have already seen to occur with yeast, which not only alters the sugar in which it grows by a ferment which it excretes, but also produces carbonic acid and alcohol by means of a ferment which remains within the yeast-cells so long as these are intact and only becomes liberated when these cells are broken up.

An excessive quantity of their own products is usually injurious to cells and too much alcohol will stop the growth of yeast. At the same time these products are frequently very nutritious for cells of a different sort and alcohol furnishes a most suitable pabulum for the organisms which produce vinegar. Vinegar in its turn is toxic to the microbe which produces it, but serves again as a soil for another which gives rise to a viscous fermentation. By the successive action of these ferments a solution of sugar may produce, first, alcohol, secondly, vinegar, and thirdly, ropy mucus. In this particular series each microbe produces a substance injurious to itself but useful to its successor. This is, however, not always the case because a cell may produce a substance not only injurious to itself but injurious to other cell, and alcohol in large quantity not only kills the cells of yeast but kills other cells as well. Similar conditions occur within living organisms where the cells composing the different parts are connected together and pass on the products of their life from one cell to another by means of the circulation of the blood and tissue juices. The secretions of one part may be, and indeed generally are, useful to other parts of the organism and so long as no

part sins either by deficiency or excessive action the whole organism maintains a condition of health. But this is not always the case and health may be destroyed by (*a*) excessive, (*b*) defective, or (*c*) perverted action of one or more of the parts composing the body.

But health is even more frequently destroyed by the invasions of organisms from without. When these organisms fall upon an open wound they tend to grow and multiply rapidly, they secrete ferments and form poisons which enable them to destroy the tissues upon which they have fallen, and then finding their way into the circulation and being carried to all parts of the body they kill the animal which they have attacked.

One of the great problems of therapeutics then is to discover how best to defend ourselves against the attacks of microbes. In Hogarth's picture we see two methods by which this is done. The dog licks the wound it has received and thus removes from it any pathogenic organisms which may have lighted upon it. By insuring their absence it renders the wound *aseptic,* and *asepsis,* which is another word for excessive cleanliness insuring the absence of organisms, is one of the great measures by which the triumphs of modern surgery have been achieved. The treatment applied by the Good Samaritan to the wounds of the traveler is somewhat different, for he pours in wine the alcohol of which may hinder the germination of any microbes on the wound and thus prevent them from producing sepsis. This method, which in the hands of Lister has revolutionized surgery, is termed *antiseptic* as distinguished from the *aseptic* method used by the dog. There is no doubt that the aseptic method has got distinct advantages over the antiseptic method as applied to wounds because any substance which injures or destroys microbes will likewise injure the living cells of that part of the body to which

it is applied. For this reason the aseptic method can only be employed to a very limited extent against microbes that have already entered the interior of the body, although it may sometimes be used, as for example in the treatment of dysentery, where repeated doses of saline purgative are now given so as to wash out from the intestinal canal the microbes which give rise to the disease, and even in ordinary diarrhea, where a purgative is employed to get rid of both the microbes and the poisons they have formed. More commonly, however, we have to depend on antiseptic methods either entirely or as an adjunct to asepsis, and a study of the action of various chemical substances on microbes has led to the introduction of a whole series of antiseptics and indeed to their actual synthetic formation, the problem to be solved being how to produce a body which will destroy the microbes most efficiently and at the same time will have the least injurious action upon the body of the animal invaded. Nor is it only inside the body that the action of antiseptics is desired. The search for preservatives for milk, meat, fish, vegetables, and fruit which shall be at the same time efficient and innocuous is one constantly going on at present. Asepsis is one of nature's methods of defense. When irritating substances get into the eye a flow of tears occurs to wash them away, from the nose and respiratory passages they are ejected by sneezing or by cough, and from the stomach or intestines they are removed by the vomiting and purging to which they themselves give rise. Even in the addition of preservatives in milk we seem to be following the example of nature because Andeer has found resorcin in which is an antiseptic in the fresh milk of cows. As Metchnikoff has shown, another method adopted by nature for removing and destroying infective microbes is to bring down upon them a host of white blood corpuscles, or leucocytes, which

swallow up and destroy them. The more leucocytes that the organism can bring to bear upon the intruders the better chance it has of overcoming them. One problem, therefore, in therapeutics is to increase leucocytosis. At present we have comparatively few drugs that possess this power, cinnamate of sodium being perhaps the most active, but one of the problems to be solved is to find other substances which will do this to a greater extent than at present. The microbes on their part are ready to attack the leucocytes and fixed cells by means of toxic secretions or toxins and another of the defensive mechanisms which the organism adopts is to form *antitoxins*. as the antitodes to these toxins are generally termed. Some of these defensive bodies or alexins actually destroy the invading microbes themselves, while others simply neutralize the poisons or toxins they have formed. The nature of such defensive substances has been examined by Ehrlich to whom we owe much of our knowledge concerning them. It is very complicated and we do not yet know the precise mode of production of these antitoxins, but it is a curious fact that in many plants we find two poisons which are antagonistic in their action and which are to a certain extent antidotal to one another. Thus in jaborandi we have two alkaloids one of which, pilocarpine, stimulates secretion enormously, whilst the other, jaborine, paralyzes secretion, so that an extract of the jaborandi plant containing them in proper proportion might possibly appear inactive although it contained both alkaloids in considerable amount The same is the case with poisonous mushrooms which contain a poisonous alkaloid, muscarin, which produces severe irritation of the intestine and an atropine-like substance which antagonizes it. Opium likewise contains alkaloids having very different actions, some being almost purely narcotic and others purely convulsant. The animal body seems to have a won-

derful power of accommodating itself to the action of many poisons and this is very marked indeed in the case of opium. Many persons who begin with a small dose increase this gradually to an enormous extent so that they are able to take with impunity many times the ordinary lethal dose. The organism has a certain power of storing up antidotal substances within itself and Dr. Cash and I were able, by feeding animals with potash, to render them less susceptible to the poisonous action of barium, but except in the case of arsenic the organism seems to have but little power of becoming accustomed to inorganic poisons. It is different, however, in the case of organic poisons as shown by the resistance to the action of alcohol acquired by habitual topers and to morphine by habitual opium-eaters. A similar resistance may be acquired to snake-venom and to the toxins produced by microbes; and here it does not seem to be merely that the cells of the organism become accustomed to the poison, but that the organism forms an antidote, not only in sufficient quantity to neutralize the poison which is introduced, but actually in such superabundance that serum separated from the blood of an animal which has become immune to the action of snake-venom or of toxins will neutralize the effect of the venom or toxins in another animal. So great is this power that Sir T. R. Fraser has found by inoculating an animal with gradually increasing doses that it may at length completely resist the action of fifty times the ordinary lethal dose of snake-venom, and in an experiment of M. Calmette I have seen an animal which had received the serum from such an immunized animal remain healthy and well, although another one which was inoculated at the same time and with the same dose of snake-venom was dying from the effect of the poison.

When horses are inoculated with successively increasing doses of the toxin of diphtheria, their blood acquires a high

antitoxic power, and the use of the serum of such blood injected into patients suffering from diphtheria has robbed this disease to a great extent of its awful power. Hydrophobia is another disease which has been to a great extent deprived of its terrors by Pasteur's method of treatment. This differs in its plan from that used in diphtheria. In diphtheria the bacilli probably form a ferment which produces a deadly poison by exercising its digestive powers on the material it finds in the body. This poison is neutralized by the antidotal serum which is formed in a horse and is injected into the patient. In hydrophobia we have not been able to isolate the virus, but from its mode of action we suppose it to be a minute organism. This virus takes a long time to act in man, sometimes three weeks but usually six weeks, but when cultivated successively in rabbits it becomes very virulent indeed and acts much more quickly. It apparently finds its chief nidus in the spinal cord. When the cord is exposed to air the virus gradually becomes weakened and by injecting with an extract of very weak cord on the first day and with a stronger extract on each succeeding day the human body becomes accustomed to the virus and forms its own antitoxins. Thus by the time that the poison inoculated by the original bite of the rabid animal has time to develop its action the person has become immune.

One of the most important problems of therapeutics, therefore, is to render the human body immune against pathogenic microbes, against the ferments they form, and the toxins they produce. The two examples I have already given show how the toxins and possibly the ferments may be rendered innocuous by injecting antidotal sera and thus producing what is called *"passive immunity,"* or by exciting the body to form antidotal substances itself and thus produce what is called *"active immunity."* Both these

methods have been used, and are being used, in regard to other diseases, especially in those produced by micrococci of various sorts which give rise to suppuration and inflammations. One great difficulty in the way, however, is that the antidotal serum produced by one cocus is not always efficient against the disease produced by another, and so much is this the case that it would almost seem as if an antidotal serum would require to be made for each particular patient. Nor are the sera altogether innocuous themselves because their injection may be followed not only by annoying rashes on the skin but by general swelling of the body like that from advanced kidney disease, or by painful swelling of the joints almost like rheumatic fever. Another of the problems of therapeutics therefore is to obtain anticoccic sera which will not produce any unpleasant or dangerous symptoms.

Yet another is to confer on the tissues of the body the power of resisting or destroying microbes, their ferments, and their toxins, and thus protecting themselves or in other words acquiring immunity against the diseases which the microbes would produce. In considering this question it may help us if we remember that the products of our own digestion are poisonous and if the albumoses and peptones formed by the digestion of a beef-steak in the stomach were injected directly into a man's veins they would kill him, whereas, when changed by the cells of the intestine and liver in the process of absorption, they nourish and strengthen him.

The complexity of toxins and antitoxins is easily understood when we consider that they are probably all formed by the splitting-up of albuminous molecules and thus vary enormously just as the splinters of a broken glass vary in size, shape, and in power to puncture or cut.

In my address at Moscow, in 1897, I ventured to form-

ulate the idea that immunity, natural or acquired, is nothing more than an extension to the cells of the tissues generally of a power which is constantly exercised during digestion by those of the intestine and liver. When microbes were just beginning to be recognized as the cause of infective disease, too much importance was attached to the mechanical effects which they might produce in the blood-vessels and tissues. As their mode of action became better known, this view was to a great extent given up, but though the small vegetable microbes, bacilli and cocci, have little injurious mechanical action, this is not the case with some minute organisms belonging to the animal kingdom, and such organisms of late years have become more and more recognized as causes of diseases. In elephantiasis the lymph channels become blocked by the ova of a small worm which inhabits the blood and thus the enormous swelling characteristic of the disease is produced. Within the last few years that dreadful scourge of tropical countries, malaria, has been discovered to be due to an animal parasite, and Manson and Ross have shown that the source of infection is the mosquito. By destroying mosquitoes or preventing their multiplication the disease can be to a great extent prevented, but we are still dependent upon bark, quinine, and arsenic as remedies to destroy the parasite and cure the disease. These are not invariably successful and we are still in want of medicines which shall infallibly destroy the parasite. The same is the case with other maladies where the infective microbe is of animal origin, as in sleeping-sickness, which is now attributed to a minute worm in the blood, or of vegetable origin as in ulcerative endocarditis, or of uncertain origin as in yellow fever.

But all these diseases excite much less attention than that which is perhaps more dreaded than any other in temperate climates, namely, cancer. We do not as yet know the

pathology of this disease. It has been shown that in it the cells of the affected part multiply and grow in a different manner from that of ordinary tissues. They assume a reproductive type and grow independently of the tissues of the body in which they are situated. We know that portions of carcinomatous growths may be carried by the blood-stream from one part of the body to another where they may act as new foci, but that they can only be transplanted with difficulty if at all from one animal to another. Thus it is evident that though their reproductive power is great their vitality is feeble. Therefore what one may hope for is, that though all the drugs hitherto tried have been powerless to prevent the life and growth of such tumors, yet something may yet be found which will attack and destroy them and nevertheless leave uninjured the healthy tissues by which they are surrounded. Lupus and rodent ulcer situated on the surface of the body have been successfully treated by the X-rays and ultra violet rays. These have little effect on deep-seated cancer. My friend, Sir William Ramsay, thinks, however, that the emanations from radium, which are to a certain extent soluble in water, might be administered with a view of destroying internal cancer, more especially as he has already found that they seem to have no injurious action when given to healthy animals. In the case of cancer it is certain that groups of cells take on a life of their own, and live independently of the wants of the organism as a whole. In some other diseases we find that entire organs become too active and thus injure the health of the whole body. One of the best examples of this is the thyroid gland which, when hypertrophied, produces, through the secretion which it pours into the blood, a curious set of nervous symptoms, dilatation of the vessels, palpitation of the heart, tremor, restlessness, excitement, and rise of temperature. In the disease known

as Graves's Disease these symptoms exist and may possibly be aggravated by the condition of the nervous system which causes the characteristic protrusion of the eyeballs and may even be the cause of the swelling of the thyroid itself. But that most of the symptoms are really due to the action of the thyroid secretion is shown by the fact that they may all be observed after excessive administration of dried thyroid gland.

Here we have a toxin formed within the body by the over-action of one of its parts and at present we have no satisfactory antitoxin by which we can remove the symptoms, although supra-renal gland has an action somewhat antagonistic to that of the thyroid, and this gland or its extract when administered internally in cases of exophthalmic goitre sometimes appears to be beneficial. The case is very different, however, when, instead of being excessive, the action of the thyroid is deficient. When this occurs in adults the circulation becomes poor, the skin cold, the movements of the body and the action of the mind slow, the aspect becomes dull and heavy, and the features puffy and swollen. When thyroid gland or its extract is given, all these symptoms disappear and the patient becomes healthy for the time and usually remains so as long as the administration is continued. When deficiency of the thyroid occurs in childhood, the effect of treatment is still more manifest, for the child thus affected becomes stunted both in body and mind, is dwarfish, feeble, and idiotic. Under the administration of thyroid it grows rapidly and becomes strong and intelligent and indeed develops into a perfectly normal person. The cure effected by thyroid in such cretins is one of the most marvelous achievements of therapeutics and many attempts have been made with portions of other organs or extracts of them to supply material which is supposed to be absent in various diseases.

The first instance of this method of treatment, or *opotherapy,* as it is called, was, I believe, my employment of raw meat thirty years ago to supply the body with a ferment to use up sugar in diabetes.[1] The method was reintroduced by Brown-Sequard with more success, but it was not until the use of thyroid gland and its extract that the potentialities of the method became acknowledged. It is more than eighteen hundred years since the question was asked "Who can add a cubit to his stature?" and all this time we have remained ignorant of any plan by which we could add a single inch to a child's stature. Yet it now seems possible that by the use of thyroid gland and pituitary body, children, who would be otherwise stunted, may grow not only to the normal size but even above it.

So long, however, as we do not know the chemical nature of the stubstances which exercise such an extraordinary effect upon tissue change we shall not be able to deal with them so satisfactorily as we can now, in a way that was formerly impossible, regulate the temperature in fever. The clinical thermometer not only shows us the extent to which fever is present, but it enables us to stop the application of our remedies in time so as not to reduce the temperature to too great an extent. Cold water, ice, and diaphoretics were formerly the only antipyretic remedies, next salicin and quinine were introduced, then salicylic acid was made synthetically, and being cheap was used extensively, and within the last thirty years an increased knowledge of chemical methods and of the relationship between chemical constitution and physiological action has enabled numerous synthetic products to be formed, some of which may be more useful in certain cases than the original salicylate of soda.

A great many of these substances primarily intended to

[1] *British Medical Journal,* 1873.

reduce the temperature have turned out to have a still more important action, namely, the relief of pain. There is no doubt that pain is useful as a warning against conditions which tend to destroy the organism and leads us to shun or remove these conditions to the great advantage of our health, but it is not always possible to do this and pain *per se* is one of the greatest evils that poor humanity has to bear. The introduction of antiseptics has completely revolutionized the art of surgery because it allows operations to be done with almost certain success which would in former days have almost inevitably proved fatal from unconscious contamination of the wound by disease-germs. But the greatest triumphs of surgery have only been rendered possible by the discovery of anesthetics. Previous to the work of Long, Jackson, Wells, Warren, and Simpson rapidity of operation was everything and careful but long-continued manipulation was impossible because the long-continued pain of the operation would inevitably have killed the patient. Even the minor pains of neuralgia, neuritis, and headache, though not dangerous to life, are most distressing to the sufferer. Formerly there was almost no drug to relieve these excepting opium, while now we have phenacetin, antipyrin, phenalgin, and a host of others, and chemists are daily at work preparing new and perhaps even better pain-killers.

Hardly, if at all, less distressing than pain is sleeplessness, and here again our powers of helping the patient have been enormously increased of late years. When I was a student almost the only hypnotics used were opium, henbane, and Indian hemp. The latter two were very unsatisfactory and practically one pinned one's faith on opium which had to be combined with tartar emetic in cases of fever. Then came the introduction by Liebreich of chloral, which was not only a great boon in itself but marked an

epoch as one of the first instances of rational therapeutics, the application of certain drug in disease because of its pharmacological action. Now we have any number of hypnotics, some of which are useful because they act on the nervous system itself and produce sleep without depressing the heart and can thus be given where the circulation is already weak, while others, like chloral, not only act on the cerebrum but lessen the force of the circulation, and by thus diminishing the flow of blood through the brain assist it to rest and aid the onset of sleep. Formerly when the circulation was too active the chief depressants were mercurial and other powerful purgative medicines, bleeding, tartar emetic, vegetarian diet, or partial starvation. Although these means may still be employed with advantage in proper cases, yet we have in addition a new set of remedies, viz., vaso-dilators, including nitrites, nitrates, and possibly a good many substances which dilate the vessels and lower the tension in the arteries, a tension which may be dangerous on the one side to an enfeebled heart and on the other to an atheromatous artery in the brain.

When the heart is failing we have a series of cardiac tonics and stimulants. Foremost amongst these, perhaps, may be put strychnine, the action of which on the heart was practically unknown when I was a student, and perhaps now it is hardly sufficiently recognized. At the time of which I speak, digitalis was looked upon as a cardiac depressant, and almost the only cardiac stimulant that was known was alcohol. Now digitalis, strophanthus, and a number of others are regularly used as cardiac tonics, and their power of contracting the vessels is also sometimes useful in removing dropsy. When this action is likely to be harmful to a weak heart, it may be lessened by the simultaneous administration of vascular dilators. We still, however, want drugs which will act only on the heart, or only

on the vessels. We require medicines which will diminish the cardiac action and dilate the vessels for use in high tension, such as so often occurs in gout, and we need drugs which will make the heart beat more forcibly while they cause the vessels to contract and raise the tension in cases of debility.

But prevention is better than cure, and if by modifying tissue-change we can obviate the high tension and hypertrophy of the heart which so frequently lead to apoplexy, or the atheromatous condition of the vessels which leads to senile degeneration of the brain or premature old age, we shall lessen the necessity for either cardiac tonics or vascular dilators. Some authorities claim that they can do this by vegetarian diet, limited in quantity as well as in quality, while others would treat it by a diet almost entirely of meat with liberal potations of hot water. The subject of diet is one regarding which the most contradictory opinions prevail and there is a sad want of precise knowledge upon which to base dietetic rules. We may hope, however, that the investigation at present being conducted by Professor Atwater under the United States Government, combined with that which is being carried on under the auspices of the Carnegie Trustees, will furnish the information we need.

Time will not allow me to do more than mention aerotherapeutics, balneotherapeutics, and hydrotherapeutics; the rest-cure which is associated with the name of one of America's most brilliant and versatile sons, Weir Mitchell; massage and movements which Ling and his pupils, both in Sweden and elsewhere, have done so much to elaborate and which when rightly used may be so beneficial and wrongly used so harmful. For all these branches of therapeutics we require a more exact knowledge of their action and the rules for employing them, so that even those who have made

no special study of them may employ them rightly in all diseases in which they may be of service.

Another method of cure consists in eliminating waste products from the body by rendering them more soluble and while limiting the water drunk would give lithia, piperazine, piperidine, and other substances which increase the solubility of uric acid. Before therapeutics can make much advance in this direction we must know more about the pathology of gout and tissue-metabolism generally, and we may then hope that not only will people be more free from the manifold symptoms that gout produces, but will live longer and the time of their activity, bodily and mental, will continue nearly as long as life itself. The power of increasing elimination of nitrogenous waste which urea possesses in a marked degree is shared by other substances belonging to the so-called purin group and day by day fresh bodies belonging to this chemical group are being made synthetically. Some of the new ones seem to have a greater power of eliminating waste than any we have hitherto had. The observations of Richardson, that alcohols vary in their action according to their chemical composition, and of Crum, Brown, and Fraser, that alteration in chemical constitution brings about a change in physiological action, are now beginning to bear rich fruit, and the synthetic preparation of remedies having different pharmacological properties along with our increasing knowledge of pathology gives us much hope for the future of therapeutics. More than two hundred years ago, Locke said: "Did we know the [mechanical] affections of rhubarb, hemlock, opium, and a man as a watchmaker does those of a watch, whereby it performs its operations, and of a file which by rubbing on them will alter the figure of any of the wheels, we should be able to tell beforehand that rhubarb will purge, hemlock kill, and opium make a man sleep." One of the great

problems of therapeutics is not only to know (*a*) what drugs to use in order to obtain certain effects, but to know (*b*) how to make such drugs if we have not got them at hand. The struggle for existence does not occur only between man and beast, man and man, or nation and nation, nor even between individual beasts or plants. It takes place also between cell and cell, not only between those cells which we term microbes and the cells which form the human body, but even between those which form the different parts of the body itself.

The great object of this Congress is to unify knowledge, to render evident the similarity of the laws which govern phenomena of the most diverse character, and it is therefore interesting to find that the grand problem of therapeutics is for the cell what those of religion and sociology are for the man, viz., to learn how to regulate the environment of each cell or man in such a manner that the individual shall not work for his or its own good alone, but for that of others as well, and how to restrain or destroy those which are noxious. When we are able to regulate cell-life by food, air, water, exercise, inoculations, or medicines, we shall be able to relieve or remove weakness, pain, or distress, not only from the bodies but also from the minds of our patients, to maintain health, increase strength, and prolong life to an extent of which at present we can hardly dream.

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